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Editorial Notes*

PROGRESS in archaeology is made possible only by publication of the evidence; Pitt-Rivers went so far as to say that the date of a discovery was that of its publication. Until the evidence has been published—that is, 'made generally accessible or available'—it cannot be freely used to reconstruct the remote past; the display of objects in a museum or of photographs and drawings in a temporary exhibition does not, in archaeology, constitute publication[1]. That is only achieved when the archaeologist concerned, whether as excavator, finder, or photographer, describes and illustrates the evidence in a book or periodical. But knowledge is advanced not only by the primary publication of, for instance, an excavation, but also by articles and monographs discussing special aspects or problems of prehistory. A glance through the contents of current periodicals should therefore give one the latest news from the archaeological front, for it is in periodicals more often than in books that advances are first registered.

These notes are meant to convey to the readers of ANTIQUITY a few impressions derived from three recent periodicals which happened to appear about the same time. Their cumulative effect was most impressive; here was a mass of good stuff, well produced, full of ideas freshly presented, but likely to escape the notice of many. Some of the articles of course deal with special problems; but even specialists in particular periods may be credited with a general interest in subjects outside their own sphere, and

* 1. *Proceedings of the Prehistoric Society for* 1950 (New Series, Vol. XVI); edited by J. G. D. Clark, assisted by K. P. Oakley and S. Piggott. London Agents, H. K. Lewis & Co., 136 Gower St., London, W.C.1. *Price* 28*s*.

2. *L'Anthropologie*, Vol. LV, No. 1–2 (published May, 1951); edited by Masson et Cie, 120 Bvd. S. Germain, Paris. Annual subscription, 2700 *francs*.

3. *Germania*, Vol. XXIX, 1951, No. 1–2; edited for the Römisch-Germanisch Kommission des Deutschen Archäologischen Instituts by Professors Bersu and Wagner. *Price D.M.* 16.

[1] The British record of publication during the last 30 years is one to be proud of, but like every other country we have skeletons still hidden in cupboards. The decades go by and we still await the publication of such major excavations as those at Windmill Hill, Avebury, Caistor-by-Norwich, Salmonsbury.

it is one of the functions of ANTIQUITY to maintain that interest. Few major unsolved problems, for instance, touch so many special spheres as that of the spread of the Indo-European languages; 'tracking the elusive Indo-European', says Mr Hutchinson[2], 'has been a favourite sport of archaeologists and philologists ever since the days of the brothers Grimm'. The problem has its roots far back in the mesolithic period, its trunk in the Bronze Age and its branches extend into the Iron Age and from India to Ireland. Though primarily of course a philological problem it concerns almost all prehistorians, however narrow their chosen field of research may be. Mr Hutchinson's tentative conclusion, that 'certain types of axe-hammer and axe-adze appear in the Aegean at times and in places which agree with those associated with the infiltration into the Levant of the first Indo-Europeans' is one that concerns classical scholars as well as prehistorians.

Mr McBurney breaks new ground in his 'Geographical Study of the Older Palaeolithic Stages in Europe'. As he says, such a study can only be undertaken when the data are numerous and adequate for the task, so that the geographical method, in its modern sense, has only now become applicable to the Old Stone Age. There is no reason to doubt that it will ultimately prove as valuable there as in the later periods, for it is fundamentally sound. The importance of reconstructing the climate and vegetation of past periods is now generally recognized; a crucial instance is the Sahara, where such essentially 'wet' animals as the hippopotamus and crocodile flourished during the human period. When adequate data have been collected, the application of the geographical method to the Sahara should yield results of outstanding importance; for here a relatively small change[3] in climate must have altered the character of half a continent, converting desert into steppe and parkland and filling dry wadis with perennial rivers.

Of the eleven articles in *P.P.S.*, 1950, two are excavation-reports, and five belong to that valuable and essentially British class which deals geographically with the prehistory of a region. This is not a full review, nor are we attempting to do more than record a few impressions. No more is possible within the limits of space here adopted which prohibit even the mention of several obviously important—perhaps more important—contributions.

L'Anthropologie was founded in 1890 by the fusion of the old and valuable *Matériaux* with the *Revue d'Anthropologie* and the *Revue d'Ethnographie*. A substantial part of each number is concerned with physical anthropology and ethnography. Less than half the space is devoted to original articles, the rest being given to reviews and notes. The reviews always summarize the contents and often contain lengthy and valuable criticisms. In the present number is a study exemplifying the use of a graphic method, based upon statistics, for recording the characteristic features of palaeolithic flint-work; though laborious, it seems to have possibilities. Then comes a description of rock

[2] 'Battle-axes in the Aegean', *P.P.S.*, 52.

[3] By this I mean a southward deflection of the course of the Atlantic low pressure systems which is small in relation to world-wide climatic phenomena.

pictures in the massif of Tefedest, Ahaggar (Central Sahara), some the work of a pastoral people during a more humid period. A few lines at the end record the existence, at the debouchment of the Oued Mertoutek, of ' fine neolithic sites abounding in worked flints, decorated pottery, hammer-stones, etc., with fine large tombs near by '. How often have we read such tantalizing remarks before, but how seldom is any other record published ! Yet these later sites, and the pottery in particular, may prove to be of crucial importance for African, and even perhaps European, prehistory. The authors of the article seem fully conscious of the need of studying the Saharan neolithic sites, of which there are ' vast agglomerations ' beside the old river valleys, now dry but once flowing and capable of supporting a large population. Cannot our French colleagues organize an expedition to study *only* these later sites, selecting for preference those (here mentioned) in the middle of Amadror where are thick stratified deposits containing a fossil fauna and flora sealed up beneath a layer of salt ? Until some such intensive study is made the archaeology of the Sahara will remain formless and void as at present.

But valuable as are the original articles and reviews, the best reading of *L'Anthropologie* is usually to be found in the Notes where M. Vaufrey comments upon current events. Though he is himself most interested in the earliest periods (especially of North Africa) he has long been struggling to direct the attention of his compatriots to the vast and almost neglected field of the early metal ages in France ; and in one of his six recommendations to the *Centre national de la Recherche scientifique* he calls particular attention to this need, to remove the ' humiliating reproach that France is a blank on the map for those periods ' (p. 157). If we might make some suggestions for a beginning, it would be to publish a corpus of line-drawings of the hoards of the Bronze Age. There are many other tasks waiting, e.g. to make plans of megalithic monuments including the barrow or cairn as well as the burial-chamber. Geographical studies of the distribution of certain typical objects would be invaluable to students of British and European prehistory. So would be a corpus of dated prehistoric pottery. For those who prefer to work in the open air, France with its Roman roads provides a virgin field. A regional study of the Dark Age cemeteries of Normandy is badly needed. There are as yet no archaeological air-photographs of any site in France. No one realizes these deficiencies more fully than M. Vaufrey himself, who has pointed them out often in print ; but the pioneer critic is always a voice crying in the wilderness and a target for obstructionists. The purpose of this paragraph is to assure his readers that M. Vaufrey is merely voicing opinions commonly held (but often courteously withheld) in this and other countries, where work on the lines indicated has been part of the normal routine of every archaeologist for several decades. He is only ahead of his times in France ; it is not he who is out of step in the march of progress.

Germania is the organ of the Romano-German department (Kommission) of the German Archaeological Institute. The long and honourable history of these bodies is part of the history of modern archaeology. The last of the regular series bore the reference number of Jahrgang 27 and purported to be for July-October, 1943, though appearing in 1949. In 1950 Professor Gerhard Bersu was invited to take up again the work which he was doing until Nazi *schweinerei* put an end to it. Professor Bersu had been Director of the Frankfurt Institute, the headquarters of the Römisch-Germanisch

Kommission. At that time Wiegand was head of the whole organization (the German Arch. Institute), whose headquarters were in Berlin. When he died before the war he was succeeded by a Nazi stooge whose name I do not remember. Bersu is now Director of the Institute; but as it cannot function completely under present conditions he is working at Frankfurt on Main. The Kommission's building was gutted by fire, but the library was saved, and as soon as the old building can be rebuilt, or a new one erected, a fresh start can be made. All archaeologists will hope that priority may be given to this work, so that German archaeology may get going again. Meanwhile Bersu is struggling to overtake arrears of publication. The years 1944-50 are covered by two parts numbered Jahrgang 28. Jahrgang 29 (heft 1–2) restarts the regular series of publications, and it is hoped eventually to resume the normal quarterly numbers. That its contents are good goes without saying; they are not only good in themselves but justly proportioned. Of the 171 pages, 66 contain eleven original articles covering every period from mesolithic to medieval. Notes and reviews follow, amongst the latter being included outstanding Italian, French, Danish, Belgian and English books. There is a long list of recent publications, followed by what is in many respects the most important section, the *Fundchronik*—a geographically arranged record of recent finds in Germany. As was said above, the publication of discoveries is of supreme importance for archaeology. That was always true; but it is truer than ever now that we have entered an era in which warfare involves not only the total destruction of museums, but also the looting of their contents by barbarian hordes.

Amongst the articles is an amusing exposure of Reinerth whose ' oldest road in the world ' proves to be an old shore-line of the lake of Federsee! The next is an account of the Chamer group of pottery in Bavaria, which has a deceptive resemblance to our grooved ware. It has what is called plastic decoration in the form of raised ribs applied to the surface. Wares of this type are widely distributed in the Neolithic period, occurring in caves in Italy, and in Spain. It obviously originates from the imitation of basketwork. Of special interest to British and Irish archaeologists, especially those who have had the good fortune to work in the field here with Professor Bersu, is the plan (opp. p. 140) of the Carolingian settlement at Burgheim, Lkr. Neuburg, on the Danube, excavated in 1949. It neatly exemplifies one of Bersu's favourite theses, namely, that pits are cellars and imply houses of wood or wattle-and-daub built above them. Again space forbids the mention of other articles. We wish the Institute every success in the new start it has made under experienced and able direction.

Jericho is not only a unique site geographically, with its perennial spring, river, salt lake and 1200-foot depression below sea level; it is also one of the oldest cities in the world. From the 6th millennium when mesolithic hunters resorted to the spring of 'Ain es Sultan down to Joshua's trumpet-blasts in the 2nd there was no break in the occupation, so that in the course of time a *tell* formed, and *tells* mean buried history. Unfortunately Jericho attracted the attention of excavators before the technique of excavation was perfected—or at any rate before it was practised east of Europe. But much remains intact, even of the exciting lowest strata; and it is good news indeed that the British School of Archaeology in Jerusalem will begin excavations there in 1952. They will be in the charge of the School's Director, Miss K. M. Kenyon, D.LIT., F.S.A.,

Lecturer in Palestinian Archaeology at the University of London Institute of Archaeology. Though we cannot, for obvious reasons, find space to appeal for support very often, we gladly make an exception here, because the excavations may produce results of decisive and far-reaching importance. The address to which readers may send their contributions is the Hon. Treasurer, British School of Archaeology in Jerusalem, 2 Hinde St., Manchester Square, London, W.1. We hope to be able to publish something about the results achieved during 1952.

The next will be our 100th number, completing the 25th year of continuous publication. Already a few messages of greeting are beginning to reach us, and we hope to be able to publish a selection in that number. The Editor himself expects to be out of reach of correspondence then, as he intends to go on an expedition (backed by the British Academy) to explore a little known part of the Nile Valley between the 4th and 5th cataracts, returning in March or April. It is hoped that before then, and soon after this present number appears, his *History of the Fung Kingdom of Sennar* (1504–1822) will have been put on sale (Bellows, Gloucester, £2 17*s*). Publication has been made possible only by the generous help of the Wellcome Trustees. The book embodies much research, and is nearly 400 pages long, with illustrations (many from the author's photographs). A small edition only is being printed. The author's interest in the subject dates from his excavation of Abu Geili for Sir Henry (then Mr H. S.) Wellcome in 1914 (see the review on pp. 161–3). All things considered the Editor feels that at the end of 1951 he will have earned a busman's holiday.

Archaeological Research in Indo-China: a review

by Erik Seidenfaden

Archaeological Research in Indo-China. Vol. I. The district of Chui-Chên during the Han dynasty. General considerations and plates, by Olov R. T. Janse, ph.d., Honorary Professor, École du Louvre (Paris). Visiting Lecturer on Far Eastern Archaeology, Harvard University.

Published in the monograph serial of the Harvard-Yenching Institute as volume vii of this serial by the Harvard University Press, Cambridge, Massachusetts, 1947.

THE author of this brilliant and monumental work is a Swedish archaeologist, whose name will be well known to those who have followed with the greatest interest his outstanding, and, from both the archaeological and historical point of view, so fruitful and important excavations in Tonkin and North Annam between 1934 and 1939. The results have appeared, from time to time, in *Bulletin de l'École Française d'Extrême-Orient*, *Cahiers de l'École Française d'Extrême-Orient*, and the *Illustrated London News*. Professor Janse's work was carried out under the auspices of the École Française d'Extrême-Orient, Hanoi, the Museums of Paris and of the Harvard-Yenching Institute. This fine book contains 113 pages in the text and 169 beautiful plates, showing a great number of the objects found, besides several clear and instructive plans of the most characteristic of the brick-built tombs excavated. There are also 56 figures in the text.

In his preface the author gives a summary of the work carried out during the period 1934–1939. Research work was not restricted to excavations only, but covered also anthropological and ethnographical studies of the Müongs, the so-called Proto-Annamites, of the Thanh-hoa province; the Man or Yao, the Meo and Thô (Thai) of Northern Tonkin and some of the Mois, the primitives of the great jungle-covered Annamite hinterland, such as the Indonesian Rhades, etc. Outside of Indochina proper Professor Janse undertook archaeological research work in Johore, Malay Peninsula, and in the Philippine Islands. A visit was also paid to the famous kitchen-midden at Somrong Sen in Cambodia, and to Kunming in Yünnan.

The principal work, however, was the excavation of a considerable number of vaulted brick tombs, mostly dating back to the period of the Han dynasty (206 B.C.–A.D. 23), among them one of the largest subterranean brick buildings ever discovered in Indochina. Several tombs of the periods of the Tang and Sung dynasties (618–907 and 960–1279 respectively of our era) were also opened and their contents examined. As a few of the tombs were found quite intact, the funerary deposits there were particularly rich and varied, including bronzes, ceramics and beads: among the bronzes were some figurines of a unique interest. The discovery of a wooden coffin was particularly remarkable, in view of the damp climate of Tonkin.

Besides the Chinese tombs other sites of archaeological interest were examined and studied, such as a kitchen midden, a neolithic site, urn burials of the first centuries A.D. and ancient Chinese pottery kilns from where large collections of pot sherds were made.

The now famous site of Dog-sön, which has given its name to the Bronze Age of Tonkin, was also studied, and here field-work brought to light several of the renowned bronze drums together with vases, weapons and beads. The rich harvest of archaeological and ethnographical objects was distributed amongst the museums in Hanoi (Musée Louis Finot), Thanh-hoa, Paris, Brussels, the Harvard-Yenching Institute and Stockholm.

In his introduction Professor Janse gives a sketch of the ancient history of Tonkin and North Annam. During the Han dynasty there was a kingdom, called Nan-yüeh, which extended from the region of Canton in Kwangsi far into the peninsula of Indochina, i.e., the present Tonkin and North Annam. This kingdom was conquered by the Chinese in the year 3 B.C. and divided into nine districts, one of which was Chiuchên, corresponding approximately to the present province of Thanh-hoa in Northern Annam. Commercial relations between this kingdom and China seem, however, to have existed already in the 3rd century B.C., or perhaps even earlier. Archaeological finds in Thanh-hoa may go back as early as that; but no real immigration of Chinese colonists began until the beginning of the Christian era. The natives of Tonkin at that time were in the neolithic stage of culture, working in their fields with hoes of polished stone. They practised irrigation; their weapons were large bows using poisoned bone-tipped arrows with which they killed pythons and other wild animals whose flesh they ate. Later on, no doubt as a result of Chinese influence, they learnt to cast bronze implements. The work seems to have been accompanied by religious ceremonies—still in vogue among some of the Southern Mois. The natives were also fond of tattooing their bodies, which is not the custom of our present-day Mois, but was until recently very much practised by the Lão or Thai of Lãos and of northern and north-eastern Siam. Also the habit, or vice, of betel-chewing was already prevalent. They wore turbans round their long hair, which was twisted in a big knot, but seem otherwise to have been scantily clothed, their country thereby deserving the name of ' Kingdom of the Naked '. The greater part of the bronze objects found hail from Dong-sön in Thanh-hoa, and they seem to have been influenced by the Chin (249–206 B.C.) and earlier Han art, and to belong to the so-called Indonesian group. This civilization, also called the Proto-Malayan or Dongsönian, has been studied by several distinguished savants, such as Franz Heger, Henri Parmentier, Victor Goloubew and the author.

The site of Dong-sön is of great importance for our knowledge about the ancient population of Indochina. The dwelling site with its necropolis must have existed long before direct contact was made with the Chinese. It was probably destroyed and burnt down by the victorious Chinese general Ma Yüan when he, in the year A.D. 43, suppressed the natives who had rebelled under the leadership of a pre-Annamite Jeanne d'Arc. At Dong-sön the majority of the tombs are ordinary earth graves, and the numerous holes in the ground show that the natives of that ancient time lived in pile-dwellings, while their descendants, the present Annamites, live Chinese-fashion in houses built directly on the ground. During the Tang dynastic period Dong-sön came to life again. Not all the rebels submitted to the Chinese though; some of them fled west- and southwards where they, no doubt, were hospitably received by the Chãms as well as by their savage kinsmen of the forest-clad hills. Professor Janse asks whether the rise of the Chãm state (present Middle- and South Annam) did not to some extent originate with the arrival of the refugees from North Annam. That is possible but the real moving force would have been the immigrating Hindu priests and adventurers. The Vo-Canh Sanskrit inscription of about A.D. 200 is no longer considered to have any connection with the Champã, and the Sri Mara mentioned is now thought to be a king or emperor of the great

Funan which at that time covered most of Indochina east of the frontiers of the present Burma.

From all that has been said so far, it seems established that the primitive inhabitants of the kingdom of Nan-Yüeh were Indonesians and as such belonged to the same ethnic group as the Chāms, the Malays, many of the Mois as well as the inhabitants of Formosa, the Philippines, and Insulinde. What then are the Annamites and the Müongs? According to the latest theories, which seem to be founded on sound anthropological evidence (Drs Maneffé's and Bezacier's researches) they both belong to the same anthropological type, the Mongolo-Indonesian. That means that the Annamites are Indonesians strongly mixed with Chinese and to a lesser degree with Thai and Môn-Khmers. The above-mentioned two anthropologists, after a careful study of the blood groups of the North Indochinese peoples and the Indonesians as far away as the Malagasy, come to the conclusion that the Annamites, the Müongs, the Thô, the Nungs (both the latter are Thai) and the Yaos come nearer to the Indonesian element than to the Mongolian.

The Müongs, the country cousins of the Annamites, differ from them in not being mixed with Chinese blood. The Annamite language has many Chinese loan-words and also some Thai elements, but curiously enough their numerals are Môn-Khmer. The Môn-Khmer elements may have entered the Proto-Annamite region about 1500 B.C., when the Mon-Khmer wave, coming from India, overflowed the Indonesian tribes who at that time inhabited Indochina, where they in their turn had replaced or absorbed a Melanesian population. The finds at Dong-sön, which in the main date from the first period of Chinese intervention in Indochina, consist of bronze spear-heads, daggers, and kos—a curious kind of dagger-axe, which is often decorated with animal pictures, among them that of the elephant. As such decorations are also found on weapons in Korean tombs we wonder whether this kind of decoration should not be connected with the very ancient animal art of the steppes, or of the nomads of Central Asia that flourished in the 3rd and 4th millennium B.C. ? The swords found seem all to be of the usual short East Asiatic type, the later long-bladed sword of the Iranian nomads not yet having penetrated to these parts of East Asia. Other objects found were long-necked bronze bottles, and the head of a bronze figurine which seems to represent an Indonesian type. We suppose that the head belonged to a kneeling statuette of the kind called a ' Chām prisoner '. As all the kneeling figurines used as lamp-stands are naked except for a *cachesexe*, they may all represent such ' Chām prisoners '.

Other finds included jade rings, bronze arrow-heads and crossbow mechanisms. It is interesting to note that some of the arrowheads are three-winged. Most of the bronze finds were made in the brick-built graves ; but besides these bronze drums a bronze bell and a fine vessel of the same metal were also found. The latter finds no doubt antedate the former. A discovery of special interest was that of a bronze dagger with a decorated blade, its handle including a rattle. According to Marcel Granet the purpose of this was to attract the attention of the gods when a bloody sacrifice was offered up.

The above-mentioned conquests of Ma-Yüan seem to have had widespread consequences besides the conquest of Tonkin and Thanh-hoa ; they probably also caused the migration of the liberty-loving Indonesians from their former dwelling places to the Philippines and even down to Insulinde. The author rejects, however, the assertions of Professor Otley Beyer, the well-known American archaeologist in Manila, who maintains that the late New Stone Age (neolithic) people of Luzon came from Indochina. We think, however, that Professor Beyer is most likely to be right, as the inhabitants of these islands (with the exception of the Aeta Negritos) are of the same ethnic stock as the

Proto-Annamites, the Chāms and many of the Mois in Indochina. The author rightly concludes that in view of what has now been accepted as a fact, i.e., that the Annamites are sinicized Indonesians or Mongolo-Indonesians, the former belief that they were emigrants from the Middle Yangtze valley must now be abandoned.

The excavation of the brick-built tombs in Thanh-hoa was the chief object of Professor Janse's expeditions to Indochina, and he excavated a large number. Treating of the structure of the tombs from the Han period the author says that they generally comprise a mound or a tumulus covering a hypogeum or subterranean construction most often built of bricks. The mound, consisting of sand or earth, had an elliptical shape and may originally have taken the form of a truncated pyramid, resting on a rectangular base with a step. This is the classical form of the tumulus in China proper during the Ch'in (249–206 B.C.) and Han dynastic periods. The mound was often oriented to the cardinal points. The Annamites of to-day build above the tombs of their ancestors' cabins for sacrificial use during the mourning period, and the Chinese of the Han period probably had the same custom ; the author thinks it possible that temples dedicated to the memory of some venerated personage may have had their origin in such mourning cabins. The hypogeum was built wholly or in part of burnt bricks ; in the latter case the roof and the walls were constructed of logs, now completely rotted away. The bricks were red, unglazed and undecorated except in a few cases. The masonry was well-made with the bricks laid by course, but no trace of mortar was observed. Was some other kind of binding material used ? Something akin to that employed by the Chāms, the composition of which is still an unsolved mystery ? Most of the brick tombs discovered by Professor Janse in Thanh-hoa have almost the shape of a funnel, or two semi-cylinders placed on the same axis. The dimensions of these vaulted tombs are generally 6 to 8 metres by 2 metres, the height from 1½ to 2 metres. There might be several chambers, even up to five ; in some cases the highest chamber was perpendicular to the end of the construction, in others the chambers were arranged in a cross. The corpse was laid in a wooden coffin, probably made from a hollowed-out tree-trunk, which is still the custom of many of the hill tribes of the Indochinese peninsula today. In some cases there are two hypogeums under the same mound ; we are probably here faced with the tombs of husband and wife. In the hypogeums of Thanh-hoa there were generally two or three chambers, of which one was for the dead person, the others for the funerary deposit and for sacrificial purposes, a kind of chapel. In some the hypogeum contained a single chamber, and then the food offerings were placed alongside the coffin. In the chapels have been found bronze vases for the joss sticks, vases for lustral water and dishes for fruit and other food. Sometimes there were consols for placing lamps to light up the burial chamber. There were doors too, but as these were of wood nothing is now left after nearly 2000 years ; still in some cases the bronze knockers have been found. These knockers wear the T'ao T'ien mask so well-known from the time of the Chou dynasty. The idea of using this terrifying mask* was perhaps to ward off from the dead the attacks of evil spirits.

Hardly any human remains have been found in these tombs ; this is partly due to the corroding soil of Indochina and partly to the custom of sending the corpses back to China to have them buried there with their ancestors—a custom still widely practised by present-day Overseas Chinese. No traces of human sacrifice have been found : this gruesome practice had probably been given up during the Han period. The funerary deposit consisted principally of clothes and objects for personal adornment such as rings

*See my paper in *Man* for January 1938—' The T'ao T'ien mask '.

and mirrors and, at times, a sword. Food and drink and miniature models of houses were placed apart in a store-room. The objects of bronze, lamps and incense burners, as well as objects of terracotta and lacquer were evidently connected with the idea of worship and sacrifice.

Describing the principal groups of the funerary objects (which must be studied with the help of the accompanying magnificent plates) Professor Janse says that the ceramic wares are the most common and consist of several kinds : a red ware used for vases, jars, cups, house models, etc. : a white ware, originally covered with a cream-coloured glaze, represented by vases, bowls, lamps, etc. ; a third kind was a well-baked blue-greyish or greyish ware, and finally there was a brownish glazed ware. While the three former kinds may have been made locally, the last one seems imported from China. Many jars were found, barrel or pear-shaped, some being decorated with the trellis or lozenge pattern, others with inlaid stones, perhaps as a substitute for necklaces of beads. The cult of jars is well-known among the Moi tribes, who regard them as spirits and believe they can marry and beget children and even speak ! Perhaps this superstition is connected with the ancestor cult. The house-models are some of the most interesting finds, for they give us a very good idea of the walled farms in which the Chinese colonists lived in Tonkin and North Annam more than 2000 years ago. The courtyards were divided into several compartments for the servants, the slaves and the cattle, while on the top of the walls were the storied houses of the masters. Sometimes there were watch-towers above the gates. Though such farmsteads were not actual fortresses, yet they could be defended both against man and wild beasts, and anyhow their manner of construction shows clearly the unsecure position of the unwelcome Chinese invader among a hostile native population. In these miniature house models have been found imitations of stoves, ladders, furniture and various household articles, all very instructive for the study of the daily life of the Chinese settlers of those ancient times.

It is significant that spindle whorls are also among the funerary outfit. As the learned author says, it is questionable whether these objects should be considered as intended for posthumous use or as symbolical, connected with the widespread belief of man's destiny being involved with the spinning maids, such as the Norns of the Asa faith and those of other religions. We should think there *is* a real connection.

In view of the rich and varied contents of the first volume of *Archaeological Research in Indochina* the second volume of this outstanding work will be looked forward to with a lively interest.

The Atecotti

by Edward R. Townsend

In his recent fine appraisal of the time-honoured ' Pictish Question ' the late Professor H. M. Chadwick[1] lends his powerful support to the theory that the first Celtic language to be spoken in Scotland, as elsewhere in Britain, was of the ' Q ' variety. He regards the Cruithnigh (distinct in this connection from the ' Picts ') as the introducers, in the Bronze Age, of this insular Celtic tongue which—he considers—was never, either in Scotland north or south of the Forth-Clyde isthmus, entirely supplanted by the modified Continental—or ' P '—Celtic of the subsequent Iron Age intruders. It is proposed, for present purposes, to accept this primary contention of Professor Chadwick without cavil. But, on a minor point, I think we are entitled to suggest that the evidence afforded by the brochs[2], by the forts constructed by means of the ' murus gallicus[3] ', and by the large hill-top oppida, all of which can with fair confidence be attributed to the Iron Age, is sufficiently strong to render it likely that, about the beginning of the present era, the descendants of the Bronze Age natives of *southern* Scotland preserved a separate ethnic identity only in the hilly interior of their country. For this reason it is proposed to limit the claim to aboriginal status south of the isthmus to Ptolemy's ' Selgovae ', who appear to have inhabited the centre of the Southern Uplands. Celtic scholars seem unusually agreed that this name is to be referred to the early Celtic vocable which was later represented by the Old Irish ' selg ', ' hunt '. A designation such as ' They-who-hunt ' was probably a sobriquet, bestowed on a primitive pastoral people by more agriculturally-minded outsiders, and is hardly likely to have been the tribal-name by which these people knew themselves. To that point we will return later. But, since the sobriquet is not clearly attested after the time of Ptolemy, perhaps we may assume that it passed out of use, as such names are inclined to do. If so, was it replaced by another ? It is in that light that it is desired to consider the question of the Atecotti. Who were they ?

All that we can say of them for certain is that—using the term in its widest sense—they were a ' British ' people who, mysteriously emerging from the darkness in the second half of the 4th century A.D., make their transient appearance on the stage of history and, in the early 5th century, vanish as mysteriously as they had come.

Most of the classical references in their regard, which constitute the only primary evidence available, have been conveniently placed together for us by W. F. Skene[4]. He provides us with two references by the contemporary Latin historian Ammianus Marcellinus, with one by the contemporary ecclesiastic St. Jerome, and with one from the contemporary military information contained in the *Notitia Dignitatum*. The first of Ammianus's references applies either to the year A.D. 364 or to 365, and it shows us the Atecotti (or Attacotti) associated with the Picts, Saxons and Scots as troublers of Romano-British peace. The second of Ammianus's references applies to the catastrophe of A.D. 367 (alternatively 368) when the defences of the Province everywhere collapsed and

[1] H. M. Chadwick, *Early Scotland—the Picts, the Scots and the Welsh of Southern Scotland*, 1949.

[2] V. Gordon Childe, *Prehistoric Communities of the British Isles*, 1940, p. 248.

[3] V. Gordon Childe, *Scotland before the Scots*, 1946, p. 136.

[4] W. F. Skene, *Celtic Scotland* (1886 ed.), vol. I, footnotes to pp. 99–102 and 106.

when the tide of invasion flowed temporarily as far south as London. It informs us that ' at that time the Picti—divided into the two nations of the Dicalydonae and the Verturiones—and also the Attacotti—a most warlike race of men—and the Scotti—wandering here and there—wrought widespread devastation '. We may note that Ammianus, by his qualifying adjectives, differentiates between each of these peoples : in his view the Atecotti were not Picts and were not Scots. This quotation is of importance because it enables us to locate the two sections of the Picts with reasonable accuracy. The Verturiones, the ' men of Fortrenn ', occupied the district lying immediately to the north of the river and the Firth of Forth. Ammianus's two Pictish divisions can be taken—as far as geographical position is concerned—to represent those two older frontier-disturbers, the Maeatae and the Caledonians. The Caledonians, we are informed, lay behind the Maeatae, and their name has been satisfactorily identified with the town of Dunkeld[5]. With the regions further to the north we are not concerned ; the evidence will show, I think, that the Atecotti are unlikely to have come from there.

The third reference—that of St. Jerome—is to the effect that while yet an ' adolescentulus ' he encountered in Gaul some representatives of the British nation of the 'Atticoti', and that they were—regrettably—cannibals. Skene, in his very comprehensive note, informs us that St. Jerome was born in A.D. 340, that he had a tendency towards applying to himself such terms as ' puer ' and ' adolescens ' at even the mature age of thirty, and that he is known to have been in Gaul at only one period of his life—at about the time of the Theodosian reconquest of Britain which immediately followed the great irruption of A.D. 367 (or 368). In his fourth reference Skene ennumerates four cohorts of auxiliary infantry who bore the title Atecotti, who were stationed in Gaul, and who must have been embodied at a time, or times, subsequent to the pacification of Britain by Theodosius. Putting two and two together, Skene concludes that of the four cohorts in question two were enrolled by Theodosius and the other two (who bore the titles Atecotti Honoriani Seniores and Juniores respectively) some thirty years later when Stilicho was acting as minister for the young Emperor Honorius.

However, the late Professor Eoin MacNeill[6] provides an amplified list of Atecottic units serving in the Western Empire. He names senior and junior cohorts of Atecotti, senior and junior cohorts of Atecotti Honoriani, and a junior cohort of Atecotti Gallicani. He postulates, therefore, a cohort of Atecotti Gallicani Seniores. And, in addition to this possible total of six cohorts, he tells us that there was a body styled simply Atecotti serving in the Eastern Empire. The number of enlisted Atecotti was, on this showing, large. Professor MacNeill also draws attention to a unit entitled ' Primi Scotti '. And, if there was a ' First Scots ', there may also, possibly, have been a ' Second '.

What are we to make of this ? It is now very generally realized that enlistments such as these were by no means the result of a rush of enthusiastic barbarian volunteers eager to bolster up the declining fortunes of the Empire, but resulted, rather, from successful Roman military reprisals. The disturbers of the peace—if they could be reached in their homeland—were rounded up and drafted for service elsewhere. On these grounds it is legitimate to surmise that the Atecotti were more exposed to such reprisals than were either the Picts or the Scots. The Roman arm, though still long, was losing much of its pristine vigour : the Picts of the north went, in this respect, Pict-free. Most of the Scotti, able to retire, if pressed, to their native Hibernia, likewise went

[5] A. MacBain, *Place Names of Highlands and Islands*, 1922, p. 142. Also W. J. Watson, *History of the Celtic Place-Names of Scotland*, 1926, p. 21.

[6] E. MacNeill, *Phases of Irish History*, 1920, p. 151.

Scot-free. But some of these Scots, being natives of Ireland, were now settled in Wales[7]: they could be got at, and it was they, presumably, who furnished the small Scottic contingent. It was on the Atecotti, however, that the storm fell most severely. I think the conclusion is justifiable that the Atecotti, IF they were natives of Britain, must be located in that region between the Walls over which the tide of Roman authority surged so often, and from which it so often recoiled. And it was here, in fact, that W. F. Skene, Sir John Rhys[8], and Sir Charles Oman[9] have all agreed to place them.

But the claims of an Irish homeland must also be considered. One English writer at least has shown much haste to disown the Atecotti on account of the mere suspicion of cannibalism. 'That they were Britons is certainly impossible,' says Mr Edward Foord[10]. '. . . The name is puzzling, but it may very well be a Latin corruption of some Irish appellation. My personal suggestion is that it may be connected with Ath-Cliath, the place which under the Vikings became Dublin'.* But in general, I think it may be said that the supposed Irish origin is due to the identification made by Irish scholars of a by-gone generation of the name of the Atecotti with that of the 'Aitheach-thuatha', the 'Rent-paying tribes' who—according to the legend—rose in the 1st century A.D., and well-nigh exterminated their (Iron Age?) aristocracy. Professor MacNeill[11], though he disowns this identification of the names, accepts the view that the Atecotti represented the socially-subordinate—or insubordinate—Irish element. And I think his views in this matter are reflected in those of the late Professor R. G. Collingwood[12]. The latter was also of the opinion that the Wall of Hadrian constituted, in Theodosius's day, a 'ne plus ultra', that the Theodosian reconquest extended no further, and that Valentia must be sought elsewhere than in the intra-mural region. The trend of the most recent evidence, however, seems to indicate that Theodosius might well have 'recovered' a Roman sphere of influence established in southern Scotland about the middle of the 3rd century and subsequently lost. And, if that was the case, it is easy to understand how the disbandment by Theodosius in this area of northern Britain of the disaffected sections of the Arcani frontier-militia may well have been followed by the appearance in Gaul of compulsorily-drafted Atecotti.

As regards the name itself—MacNeill[13] concurs with the interpretation put forward by the distinguished Celticist, Whitley Stokes. Sir John Rhys[14] was also disposed to agree. The suggested derivation is from the prefix ATE—having in this case an intensitive force—followed by the adjective COTTOS, 'old', the whole signifying the 'Very Ancient'—or 'Aboriginal'—People. Professor MacNeill notes, however, that 'the adjective COTTOS does not appear to belong to the vocabulary of Irish, but it is found in the various Brittanic dialects and was a frequent element in Gaulish nomenclature. The Atecotti, therefore, probably received their name not in Ireland but in Britain or Gaul'. But surely this tells very heavily against his theory that the Atecotti were Irish! Newly-arrived invaders from Ireland are most unlikely to have been called by Britons (or by

[7] Collingwood and Myres, *Roman Britain and the English Settlements*, 1936, p. 282.

[8] Rhys and Brynmor-Jones, *The Welsh People*, 1900, p. 102.

[9] Sir Charles Oman, *England before the Norman Conquest*, 1910, p. 161.

[10] E. Foord, *The Last Age of Roman Britain*, 1925, p. 82.

* An impossible etymology. ED.

[11] MacNeill, op. cit., p. 149.

[12] Collingwood and Myres, op. cit., p. 284, footnote.

[13] MacNeill, op. cit., p. 148.

[14] Rhys and Brynmor-Jones, op. cit., p. 102.

Gauls) the ' Very Ancient ' people. The name was given in Britain, by Britons, and to a people they regarded as being more genuinely ' ancient ' in the country than they were themselves—and to whom, as we have seen, St. Jerome attributed one peculiar ' ancient ' practice. How widespread was that practice ? It must nowadays, I think, be admitted that a charge of practising ritual anthropophagy would lie not only against the ancient Irish[15], and also some sections of the Romano-Britons[16], but, in fact—and at no very distant date—against the ancestors of most of the nations of western Europe[17]. In matters such as these the anthropologist and the archaeologist seem to be forcing on us the necessity for a gentler and a more just appreciation of the peccadilloes of our forefathers than any that could have been possible only a few years ago. ' Beaucoup comprendre, c'est beaucoup pardonner '.

It seems, therefore, that the Atecotti were natives of Britain rather than of Ireland, that they are best located somewhere between the Walls, and that they must represent an older element in the population than that furnished by the intrusive Gallo-Britons of the coastal areas. The names ' Selgovae ' and ' Atecotti ' both appear to contain an implication of ' primitiveness ' ; the first name seems to disappear in the 2nd century A.D., the second to first appear in the fourth. It is suggested that the one sobriquet replaced the other.

As regards the tribal—or ' national '—name used by these folk themselves, we may note Professor Chadwick's suggestion[18] that the name ' Cruithentuath ' may perhaps be discernible in a corrupt Latinized form as ' Corionototae ' in the inscription found at Hexham Abbey commemorating the slaughter by Roman cavalry of a band of raiders. He is, further, of the opinion that the Ravenna geographer's inter-mural place-name ' Coritiotar[19] ' may contain, once again corruptly, the same name's essential part. Perhaps, therefore, the ' Creenies ' of Galloway folk-tradition, referred to both by Dr W. J. Watson[20] and by Professor Chadwick,[21] may reflect a downward movement at a much later date of hillmen from the central region into the coastal plain.

The whole of southern Scotland was for a period, commencing in the 5th—or possibly the late 4th—century A.D., under the political domination of Romano-British princes. It is not surprising, therefore, that even in the central shire of Peebles Welsh place-names should occur, though they are in a small minority as compared with Gaelic ones[22]. Need these Welsh names be so very old ? Penteiacob, an older name for Eddleston, may signify in Welsh the ' Head of Jacob's House[23] '. But there can have been no ' House of Jacob ' at Eddleston until Jacob himself took out—as it were—naturalization-papers in the island. Such a name must be post-Christian. And if, in fact, Jacob supplanted Esau, our old hunter, in the birth-right that was his brother's, it was to profit him but little.

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[15] R. A. S. Macalister, *The Archaeology of Ireland*, 1949, p. 166.

[16] Macalister, op. cit., p. 285, footnote.

[17] Grahame Clark, *Archaeology and Society*, 1939, pp. 185–7.

[18] Chadwick, op. cit., p. 71.

[19] Chadwick, op. cit., p. 72.

[20] Watson, op. cit., p. 178.

[21] Chadwick, op. cit., p. 156.

[22] Watson, op. cit., pp. 132 and 135. But Dr Watson's assessment of Welsh names may perhaps be unduly high. See the late Dr E. J. Gwynn, *Scottish Gaelic Studies*, vol. II, part 1, June, 1927, p. 102.

[23] Watson, op. cit., p. 135.

A Bronze Ornament of Western European Origin, found in Northern Norway*

by THORLEIF SJØVOLD

Tromsø Museum

THE Viking raids on Western Europe are well-known from written, historical records. It has been suggested, however, that if those sources had not existed, we should have been obliged to reconstruct the history of the Vikings on a foundation of purely archaeological evidence. Typically Scandinavian graves on Western European territory bear witness to visits by Scandinavian people; and numerous finds of Scandinavian weapons also give some indication of the purpose of the visits.

As a supplement to this, we find in Scandinavia a series of contemporary objects of Western European origin, especially and not least in our grave finds. The bulk of them are personal ornaments of different kinds, but it is a point of interest that few or none have originally been designed for such use. As a rule, the objects have originally been used as mountings for holy books, reliquaries and other things; then they have been torn away and fitted with pins to be carried as brooches, etc.

At the expense of the Norwegian Scientific Research Fund of 1919 (*Forskningsfondet av* 1919) this great and interesting material has recently been published in a great work: 'Viking Antiquities in Great Britain and Ireland',[1] edited by Haakon Shetelig, who has also written an introductory volume. The materials from Scotland, Ireland and England are treated in separate volumes; and a fifth volume, Jan Petersen: 'British Antiquities of the Viking Period, found in Norway', gives a complete review of British imports in our Viking finds.

Archaeology, however, is never at rest. While this book was still in preparation, a new find of considerable interest was acquired by the Tromsø Museum: a bronze mounting of British origin, used as a brooch. It is of interest as a piece of work of great artistic value, and also because it was found in Hillesøy in the parish of Lenvik where may have resided the famous viking Ottar (Othere), the man who furnished king Alfred with so much information concerning Northern Scandinavia that Alfred found it worth while to include it in his Anglo-Saxon version of Orosius.

The brooch (as I shall henceforward call it) was found by Dr Gutorm Gjessing and is mentioned by him in a paper discussing some commercial problems of Northern Norway in the Iron Age,[2] where, however, it is treated chiefly from the point of view of commercial history. I therefore presume that there is still a need for a more detailed, analytical description of the brooch, from the point of view of style history, for the benefit of those who might be inclined to regard it preferably as a record of such.

The brooch was found in a woman's grave, and, like many other brooches of western European origin, it has been used as a 'third brooch', in connexion with two

* This article has been published previously in the Norwegian language in the year-book of the Stavanger Museum (*Stavanger Museums Årbok* 1946) and is printed here in English by kind permission of the director of that museum, Dr Jan Petersen.

[1] Reviewed in ANTIQUITY no. 85, March 1948.

[2] 'Noen Nord-Norske handelsproblemer i jernalderen', *Viking* III, Oslo 1939.

oval bronze brooches. These two brooches are of the same type as Rygh : Norske Oldsager, No. 647, the ' pure ' variety,[3] and thus dates the grave to the 9th century.

As will be seen from the PLATE, the brooch is round. It is also slightly convex. The diameter is 8.9 cm. ($3\frac{1}{2}$ inches). It is cast in bronze, and the surface is gilt. In the centre, there is a round, rifled knob of gilt bronze, and round the circumference there were 4 knobs of the same kind, but one of them is now missing. The knobs were cast separately and riveted to the brooch.

The entire surface of the brooch is covered with ornament, the separate motives of which may be of different provenance and character ; but in spite of this the general appearance is excellent. And although it may give a somewhat distressing impression at first glance, it will soon be discovered that the entire composition rests upon a thorough orderliness and an unbreakable symmetry, in great things as well as in small ones.

FIG. 1

Starting with the circumference, we first find an edging of ribbon interlacing. It is divided into 4 parts of equal length by the before-mentioned bronze knobs, and together with them it forms a firm frame around the chief part of the composition. This consists of two pairs of opposing animals, divided by palmette-like plant-motives. Finally, in the centre there is a small, flat field, ornamented by fine, engraved interlacing lines.

The ornamentation thus consists of three chief parts : ribbon interlacing, animal and plant motives. We then proceed to a somewhat closer examination of each of these motives.

First, then, we have the edging (FIG. 1), consisting of 4 equally long, separate parts. For sake of brevity, I have marked the parts with the letters *a-d*, and it will soon be seen that parts *a* and *c* are identical while the parts *b* and *d* may be termed variations on the same theme.

The simplest motive is that of part *b*, the substratum of which is an interlacing of the very simplest kind, consisting of two ribbons only. This, however, has evidently not given this part of the edging a sufficiently firm character, and to tighten it the artist

[3] Jan Petersen : *Vikingetidens smykker*, fig. 37 : 1.

BRONZE ORNAMENT USED AS A BROOCH. FOUND AT HILLESØY, PARISH OF LENVIK, NORTHERN NORWAY. Scale about 3 : 2

facing p. 128

has contrived to double the number of ribbons. To this end he has introduced a figure like a horizontal 8 at both ends of this part. Furthermore, the basic two-ribbon interlacing is not drawn continuously throughout the whole field. Immediately before it reaches the horizontal 8 at the opposite end of the field, the ends of the ribbons are joined, and the interlacing comes to an end. The result of this is that even in the middle of the field we get a figure rather like a horizontal 8, but somewhat larger.

A ribbon interlacing pattern identical with this one is to be found on the famous ' Tara Brooch '[4] in the Dublin Museum ; and a very similar one is also to be found on a piece of work of undoubtedly Scandinavian origin, to wit the well-known rectangular bronze brooch from Skabersjö in Scania.[5]

In part *d* the foundation is the same as in part *b*, but here the ribbons are split. The interlacing thus gets a somewhat more complicated character, and neither is it so strictly regular as in part *b*.

Fig. 2

The parts *a* and *c* are ornamented in exactly the same way, as has already been stated, the motive here being a two-ribbon interlacing of the so-called ' Irish knot-style '. This motive is very common, on purely Scandinavian as well as on foreign pieces of work. There is no reason here, however, to enter upon a prolonged discussion of the origin and distribution of the ribbon interlacing ornamentation. This has been done before, lately by the Swedish archaeologist Pär Olsén[6].

Nor shall I give a more detailed discussion of the plant or animal motives of the brooch. Both of them are foreign elements in our Viking age ornament. Plant ornamentation does not occur at all so early in our Viking age styles. In Anglo-Saxon art, however, there often occur plant motives, the details of which may be more naturalistic than those of the Hillesøy brooch (fig. 2), but the composition may be quite the same. I may mention a vine plant in the Vatican Gospel-book[7] where the stem is jointed in

[4] Romilly Allen : *Celtic Art in Pagan and Christian Times*, plate opposite p. 226.

[5] Figured in several works, e.g. Bernhard Salin : *Die altgermanische Thierornamentik.*

[6] Pär Olsén : *Die Saxe von Valsgärde*, Uppsala 1945, p. 92 ff.

[7] Johs. Brøndsted : *Early English Ornament*, fig. 97.

the same characteristic way, and the branches arranged in exactly the same strictly symmetrical manner. On the 'Alfred Jewel'[8] in the Ashmolean Museum, Oxford, there is a stylised vine plant which is very similarly composed, and, to quote another piece of metal-work, I may also mention that on the 'Ormside Bowl'[9] plant motives of a closely related character are to be found. On the stone crosses, similar motives also occur.[10]

The animals too (FIG. 3) seem to have their closest analogues in Anglo-Saxon art. On the silver pins from the river Witham[11] some winged animals of a very related type occur; and such a characteristic detail as the transformation of the hind part of the animals into an ornament similar to the plant motives, is found on a M.S. of the Canterbury School,[12] the Gospel-Book in the British Museum (Royal I E VI).

The chief parts of the ornamentation thus clearly point to England as the place of origin of the Hillesøy brooch. It may also be mentioned that in Anglo-Saxon art there

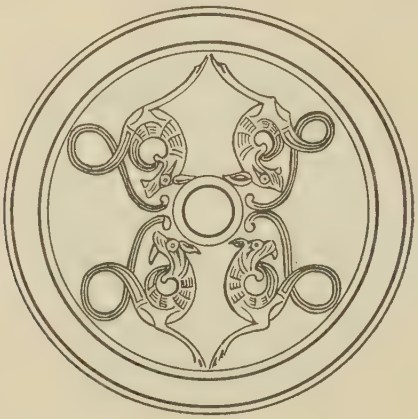

FIG. 3

occurs a bronze brooch,[13] from Alton Hill, Bottisham, Cambs, the composition of which reminds us very strongly of the Hillesøy brooch. Here, too, there is a chief field ornamented with 4 animals (which, it is true, are of a very different character); and around this there is an edging of ribbon interlacing, divided by knobs into 4 parts of equal length. In the centre, there is a similar, but somewhat larger knob, but all the knobs here are made of bone instead of bronze.

The problems of style history arising from the Hillesøy brooch cannot be further discussed here. In the Viking age material of Norway the brooch is an alien, and I therefore have found it sufficient to suggest its provenance. As far as I can see, there can be no doubt that it originates from England; and it is the task of British archaeology to find its place in the style history of British Dark Age history and to evaluate its importance in its own settings.

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[8] ibid, fig. 117. [9] T. D. Kendrick: *Anglo-Saxon Art to* A.D. 900, plate LX.

[10] ibid, plate LXI. [11] Brøndsted, loc. cit., fig. 113; Kendrick, loc. cit., pl. LXXI.

[12] Brøndsted, loc. cit., fig. 86.

[13] Baldwin Brown: *The Arts in Early England*, vol. III, plate LXIII, 1.

'Libyan' Forts in South-west Cyrenaica

by R. G. Goodchild

In his comprehensive study of the *Eastern Libyans*, published in 1914[1], Oric Bates devoted several pages to the discussion of certain forts, seen in south-west Cyrenaica by 19th-century travellers, the architecture of which seemed to indicate a native Libyan origin, entirely free from Greek or Roman influence. After repeating the descriptions given by these travellers, and reconstructing plans and sections of two of the forts from the recorded measurements (always a perilous undertaking), Bates concluded that these structures, typified by Gasr el-Heneia near Agedabia, and by several examples near Ghemines, belonged to the ninth or eighth century B.C., 'the great era of polygonal masonry'.

That the ancient Libyans had no cities, but only fortified strongholds, situated at water-points, is attested by Diodorus, who adds that these strongholds were used as deposits for surplus crops[2]. It would be surprising, however, if a semi-nomadic race, as backward as Diodorus paints the ancient Libyans, had the inclination and competence to build such massive forts as those which Bates discusses. If such competence had, in fact, existed, we might be led to conclude that classical writers had not done justice to the early Libyans, and that Greek colonisation followed by Roman political absorption had stifled a native culture of considerable promise.

Bates himself urged that these Cyrenaican forts should be further investigated, but various factors, including the unsettled political situation in Cyrenaica[3], prevented any such investigation from being carried out during the years of Italian occupation (1912–42). It seemed necessary, therefore, during the 1950 field campaign of the Map of Roman Libya committee[4], to test this hypothesis of Bates by a re-examination of the actual structures on which it was based. If these structures appeared to be as early as was claimed, they would clearly have to be omitted from the Roman map; if, on the other hand, they were to prove to be of Roman date (as certain comparative evidence from Tripolitania suggested), they would have to be included. As the full programme of the expedition was an extensive one, ranging over the whole area of ancient settlement from Sirte to Derna (a distance of some 500 miles), it was clearly impracticable to attempt excavations on the sites in question. It was hoped, however, that a study of the surface remains might throw some light on the problem.

The first of these sites to be visited was Gasr el-Heneia, which stands, on a slight rise, in a desolate plain of typical Syrtic character, five miles south of Agedabia. Hamilton had visited this ruin in 1852, and had described it as 'a strong fortress of very early architecture, and by far the most curious construction I had met with in these

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[1] Oric Bates, *The Eastern Libyans* (London, 1914), 160 seq.

[2] *Diodorus Siculus*, III, 49.

[3] The area between Ghemines and Agedabia was not brought effectively under Italian control until 1923, whilst the bottom of the Syrtic gulf was only pacified in 1928.

[4] The archaeological serveys of Gasr el-Heneia and the Ghemines forts were carried out by Messrs M. Ballance, R. McGregor, D. Smith, J. Spaul, and P. Titchmarsh in July, 1950, under the direction of the writer. We are indebted to the Government of Cyrenaica for many facilities, including accommodation at Agedabia and Ghemines, and to the British military authorities.

countries . . . I have no hesitation in ascribing it to a date coeval with the best monuments in Cyrene '[5]. Rohlfs, visiting Heneia in 1868, decided that the fort ' was neither a Greek nor a Roman construction, but of Libyan origin[6] ', which conclusion, as we have already seen, was taken up by Bates and introduced into his book, together with supposedly confirmatory evidence from other sites at Ghemines. The only other first-hand

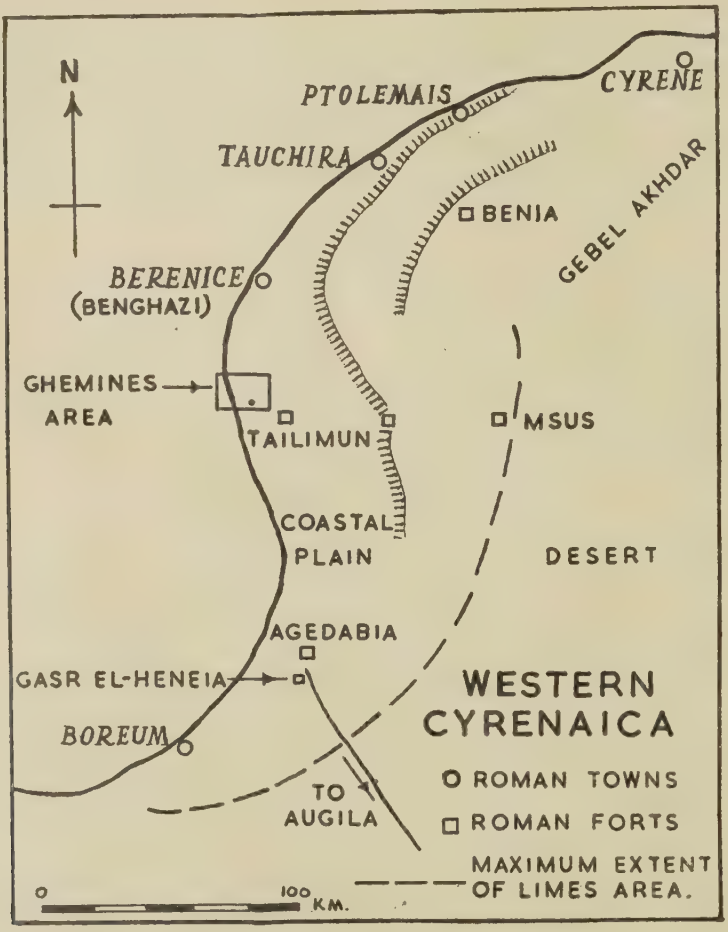


Fig. 1

record of Heneia appears to be that published in the report of the Bodrero topographical mission which explored large areas of Cyrenaica in February-March 1919, after the first ephemeral Italo-Senussi pact[7]. This description is a summary and non-committal one,

[5] J. Hamilton, *Wanderings in North Africa* (London, 1856), 175–6.

[6] G. Rohlfs, *Von Tripolis nach Alexandrien* (Bremen, 1871), vol. II, 39.

[7] *Itinerari della Cirenaica* (*Note ed appunti della missione Bodrero, Feb.-Mar.* 1919. (Rome, 1920) 76 and fig. 99–101.

but is accompanied by some good photographs, one of which has been reproduced, without comment, by Fantoli in his collection of ancient geographical texts relating to Libya[8]. No plan of Heneia, other than Bates' hypothetical reconstruction, appears to have been published, nor are these earlier descriptions sufficient to give a clear idea of the character of the structure. A full description may therefore be useful.

Gasr el-Heneia appears from the distance as a low mound, from which protrude some fragmentary walls, not more than two metres high. Only when one approaches close to the site does its defensive character become evident, the mound proving to be an 'island' 23 metres square surrounded by a vertical-sided rock-cut ditch, 4–4½ metres wide (FIG. 2). This ditch must originally have been about 5 metres deep, as is shown by the level of the doorways which open into it, but it is now much silted up (FIG. 3). The rock wall of its inner face is pierced by small triangular light-slits which served to illuminate the rock-cut basements which lie beneath the fort. These slits may also have served as loop-holes.

Hamilton describes how the fort 'was approached by means of a wall, hardly fifteen inches broad, which is built across the moat on one side. This wall was, perhaps, once the support of a movable bridge'. The wall in question, which still spans the northern ditch, is in fact rock-cut and not of built-up masonry; and Hamilton's explanation of its function is entirely wrong. The actual entrance to the fort consisted of a wooden bridge[9], supported on twin stone arches, spanning the southern ditch. The recesses for the horizontal beams, and for the lower voussoirs of the arch, are still clearly visible in both faces of the ditch (PLATE II, d). One of the voussoir recesses on the outer face of the ditch cuts rather awkwardly into a rock-cut chamber and it is possible that the supporting arches were a later addition to a simple wooden bridge; but this possibility should not be unduly stressed.

The outer walls of the fort, 1.50 m. wide[10], form a square of 16 m. externally; they are built of medium-sized stones, reasonably well coursed, but have largely fallen or been robbed, so that only the north-east and south-west corners are visible. Between the outer face of these walls and the inner edge of the ditch is a berm 3.50 m. wide with a low revetment wall on the edge of the ditch (PLATE I, a): it was probably the large, irregular, blocks of this revetment wall which gave Rohlfs the impression of an archaic form of architecture. Of the inner arrangements of the fort at ground-level nothing is now visible owing to fallen masonry, the most conspicuous feature being the rectangular staircase well, serving also as a light shaft, which penetrates into the basements. A rock-cut staircase descends from ground-level on the west side of this well, and there are indications of another narrow staircase descending from the east side. The cutting of these stairs put out of use an earlier staircase which had descended from the south side of the well (PLATE II, c).

It is only when we descend the staircase into the basements that we begin to appreciate the complexity of the lay-out (FIG. 4) of Gasr el-Heneia, and the justice of Hamilton's description of the fort as 'the most curious construction' he had encountered during his long travels in Libya. Opening off the bottom of the well, by doorways which are

[8] A. Fantoli, *La Libia negli scritti degli antichi* (Rome, 1933) pl. opp. p. 140.

[9] The ditches which surround the Roman towers and fortified farms of Libya are usually uninterrupted, and bridges must have existed in most cases. Gasr el-Heneia does, however, provide the first evidence of their existence.

[10] The thickness of these walls probably indicates that the building had an upper storey, and was a high watch-tower.

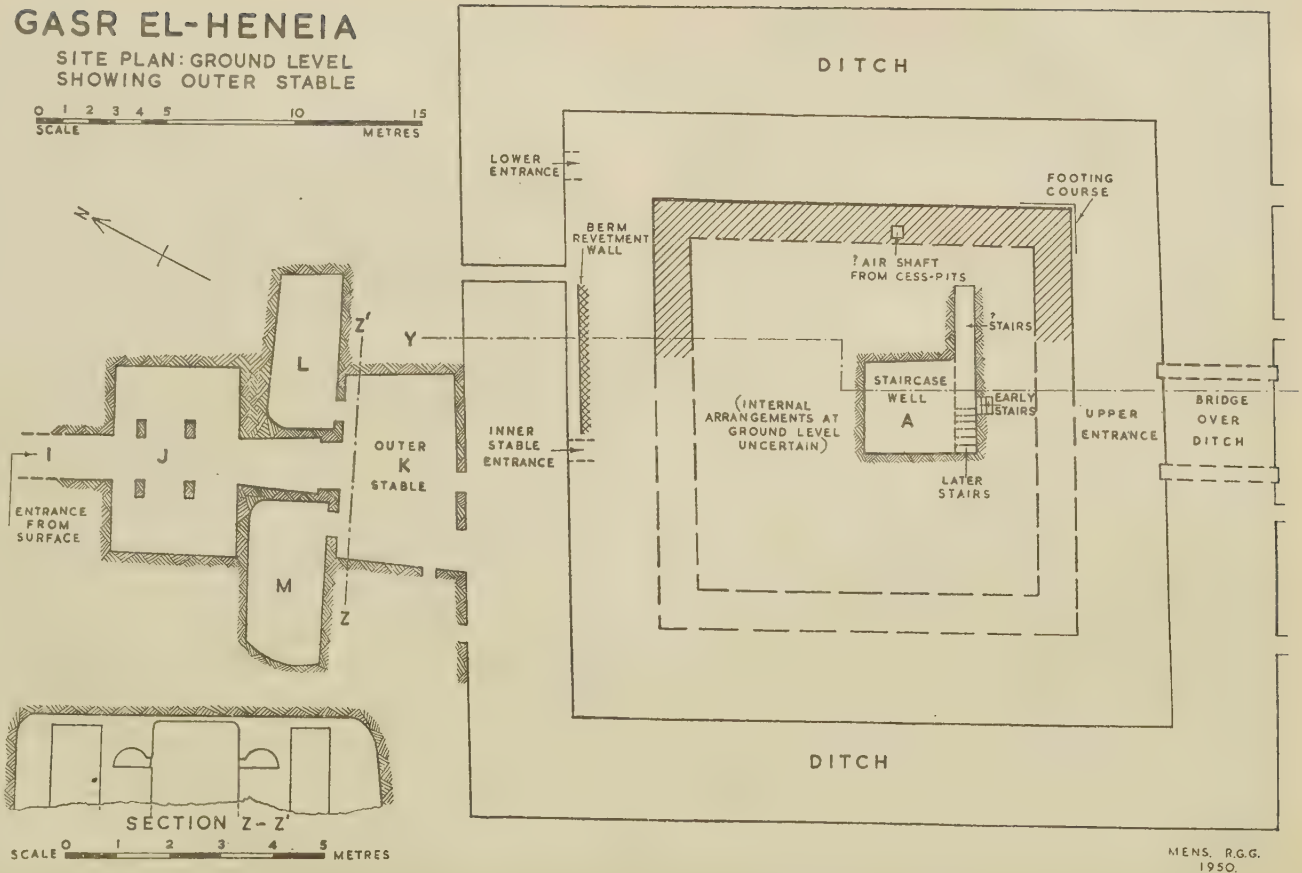


FIG. 2. (THE 'OUTER STABLE' AND CONNECTED ROOMS ARE BELOW GROUND-LEVEL)

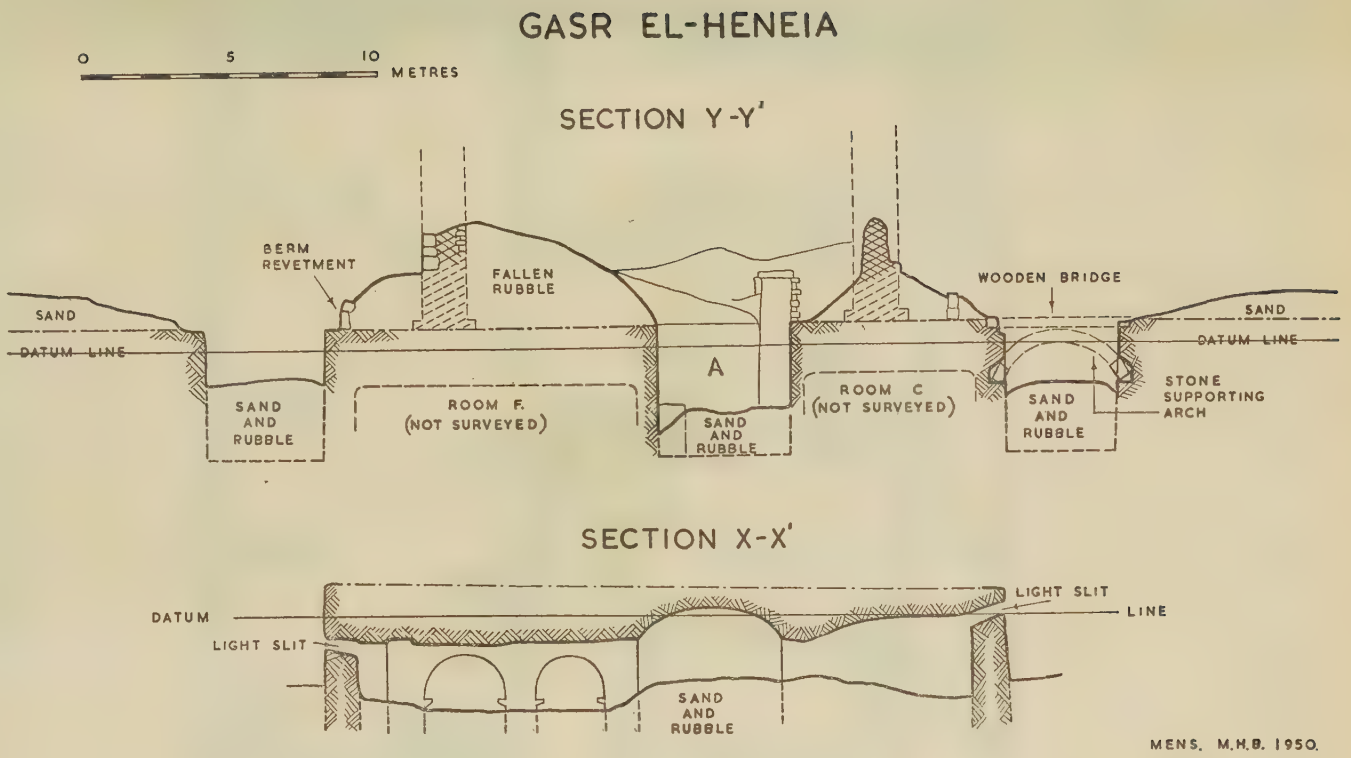

GASR EL-HENEIA
0
5
10
METRES
SECTION Y-Y'
BERM REVETMENT
FALLEN RUBBLE
WOODEN BRIDGE
SAND
DATUM LINE
SAND AND RUBBLE
ROOM F. (NOT SURVEYED)
A
SAND AND RUBBLE
ROOM C (NOT SURVEYED)
SAND AND RUBBLE
STONE SUPPORTING ARCH
SECTION X-X'
DATUM
LIGHT SLIT
LINE
SAND AND RUBBLE
MENS. M.H.B. 1950.

FIG. 3

now damaged and partly blocked by sand and fallen stone, are a number of remarkable chambers, all cut in the living rock, which occupy the whole basement area of the fort.

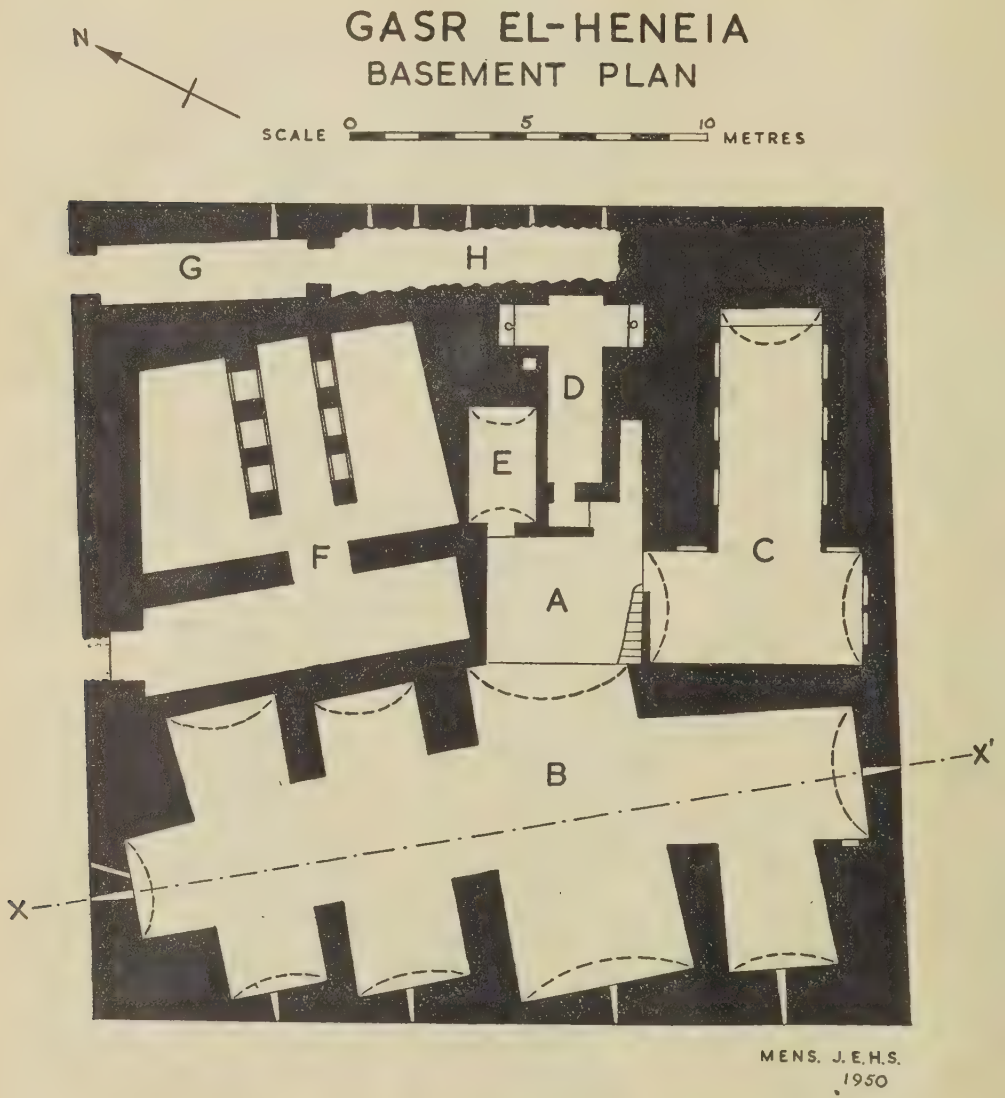


Fig. 4

Although the rock-walls of these chambers are cracked by earth-tremors, and have been pierced in places by treasure-hunters, the general arrangements of these basements are reasonably clear. The lay-out was doubtless intended to be rectangular but, as so often

happened in ancient rock-cutting, the alignments were gradually lost, with the result that the long gallery (B) is considerably askew to the axis of the fort[11].

This gallery is now heavily blocked with sand (PLATE I, b), but must have been impressive in its original condition. The division into vaulted bays, each lit by a light-slit opening into the ditch, was no doubt dictated by the necessity of supporting the weight of the rock-ceiling. In all probability the gallery provided the barrack and messing accommodation for the horsemen whose mounts, as we shall see, were stabled close at hand. A smaller, T-shaped, room (C), the walls of which had semi-circular niches cut in them[12] may have served as office or officers' quarters. Most remarkable is the latrine (D)—perhaps the only ancient rock-cut example still preserved intact—which has two stone seats in opposite bays, each equipped with small semi-circular niches for lamps (FIG. 5). At the foot of each seat were cess-pits covered with stone slabs; a vertical airshaft, visible in the masonry of the east wall, may have served to ventilate these pits. Adjoining the latrine, but with a separate entrance into the staircase-well, was a small chamber (E) of uncertain purpose.

Also forming part of the basements, but approached independently by means of doors leading from the ditch, were the 'inner stables' (F) and the 'lower entrance corridor' (G). The stables consist of a vestibule leading into the stalls, which have a flat rock ceiling supported on two rows of rock piers. Between the piers are troughs, from which it seems that six horses could be stabled, in rather cramped conditions, in the stalls, and fed and watered by a groom working in the central corridor[13]. The lower entrance corridor, entered by a doorway in the north-east corner of the fort ditch, rises gradually and leads into a second corridor (H) into which the expedition was not able to penetrate, owing to later blocking. Whether this second corridor led up to the berm, or made a right-angled bend which enabled it to enter inside the fort walls, could not be ascertained without extensive excavation. It is certain, however, that the purpose of the lay-out was to provide access between the main part of the fort and the ditch.

These complicated arrangements become more intelligible when we study the galleries cut into the outer edge of the ditch. These are still visible and accessible in the northern and southern ditches, and it seems probable that other chambers, now sanded up, are to be found in the eastern and western ditches. The galleries in the southern ditch are entered by four small doorways ranged symmetrically in relation to the bridge: they are simple rock-cut chambers which offer no features of special interest, except insofar as they indicate a need for living or storage accommodation greater than could be provided by the fort proper. In the northern ditch the rock-cut galleries are more complex and interesting, and incorporate the passage by which horses were brought down from ground-level outside the fort into the ditch, and thence into the inner stables.

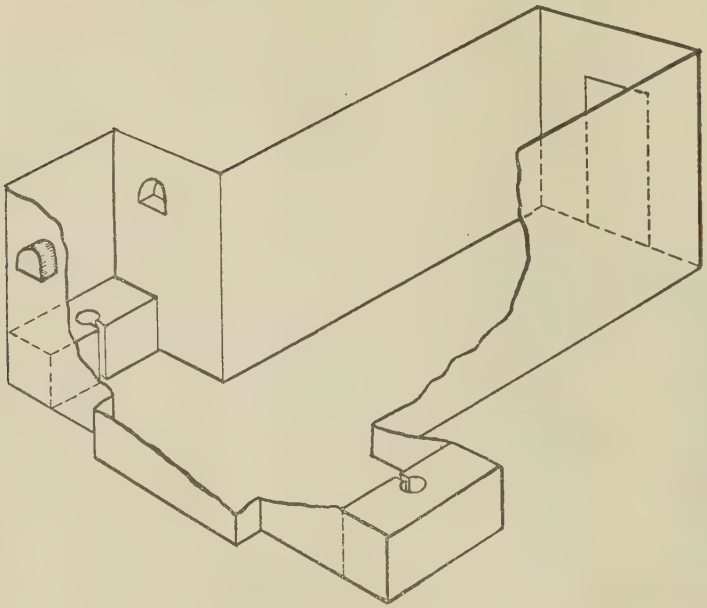
Some 16 metres from the outer edge of this ditch a depression in the ground (FIG. 2–I) marks the sloping ramp which led down into a rectangular underground chamber (J) with its roof supported on four rock piers. From this chamber a passage leads into the outer stable (K) in the ceiling of which are stone bosses perforated to receive halters. The inner face of the doorway leading from J into K has curious semi-circular slots in each

[11] The plan (fig. 4) was made by Mr John Spaul in conditions of considerable difficulty, owing to lack of light, and to intrusive sand and rubble. The alignments of rooms C, D, and E could only be ascertained approximately.

[12] Niches of this type are frequently found in the Roman fortified farms of the Tripolitanian desert area. They probably held lamps.

[13] It is, perhaps, unlikely that this small and unlit stable was ever used except in an emergency.

GASR EL-HENEIA. ROCK-CUT LATRINE.



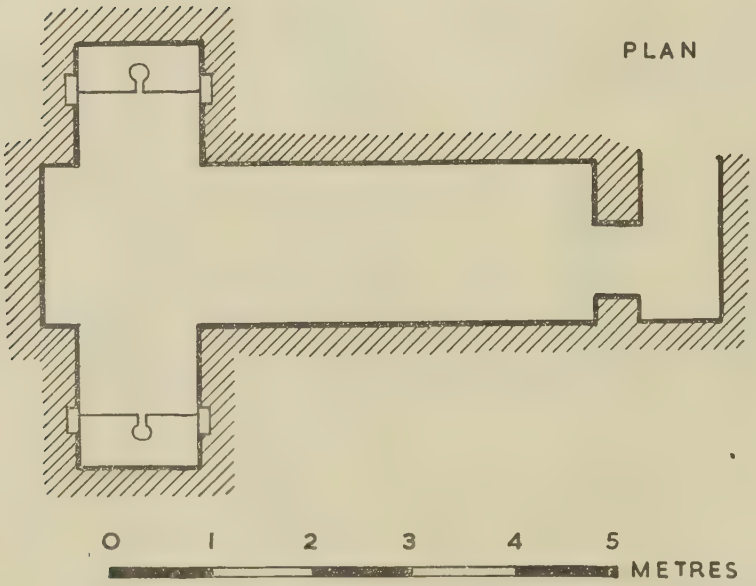


Fig. 5. (THE NORTH POINT IS ON THE LEFT OF THE PLAN)

jamb, which suggests an elaborate system of barricading this door against attack from outside. On each side of the door are guardrooms (L and M) communicating with the outer stable. Two doorways lead from the stable into the ditch.

When we correlate these features in the outer side of the ditch with these of the fort basements, we begin to appreciate the ingenuity of the whole defensive system of Heneia. Despite its small size the fort could accommodate mounted detachments, and could, in time of emergency, resist siege. The dismounted members of the garrison entered the fort by the bridge across the southern ditch, but horses were brought in, at a lower level, from the north side and tethered either in the outer stable or in the ditch itself, in conditions of excellent security. In an emergency a limited number of horses could be stabled in the basement of the fort.

Since so much of the daily life of the fort was carried on in the deep flat-bottomed ditch, it was essential to provide access from the ditch to the interior of the fort. Direct access through the inner stable would have been perilous, since the outer stable offered attackers the best means of penetration into the ditch. By leaving a rock-barrier across the whole width of the ditch to the east of the outer stable entrance, and by cutting the lower entrance beyond this barrier, close to the north-east corner of the ditch, this danger was avoided. Attackers who had managed to fight their way, through the outer stable, into the ditch could possibly penetrate into the inner stable[14], but they were still far from penetrating into the bowels of the fort itself. To achieve this they would have had to go round three sides of the ditch, exposed all the way to fire either from the parapets, or from the light-slits of the basement. The lower entrance could also be used by the defenders as a 'sally-port' from which to clear the ditch of any attackers who had penetrated into it: the blocking wall protected their rear in the course of such sallies.

These defensive features, which must have made Gasr el-Heneia a fortress as nearly impregnable as one can conceive, are clearly the work of intelligent minds and designed against a determined enemy. To what period and race are we to attribute them? To the ancient Libyans, as Rohlfs, followed by Bates, supposed; to the Greeks, as Hamilton suggested; or to the Romans and Byzantines whose military works are by far the most common in Libya?

The ancient pottery found scattered on the surface outside the fort, and to a lesser degree within it, is completely Roman in character, but it might be argued that such surface pottery represents only the latest phase of occupation[15]. There are numerous graffiti scrawled on the walls, of which the earliest are Arab tribal marks, and the latest the names of British soldiers of the last war.[16] Over three of the four doorways cut into the outer side of the eastern ditch are markings which possibly belong to the period of the fort's occupation. They are reproduced below (FIG. 6) but it must be confessed that their meaning is completely obscure. More suggestive is the architecture of the building: the stone arches which supported the bridge over the eastern ditch[17], the round vaulting of the basement chambers, and the advanced sanitary arrangements, all indicate

[14] A diagonal loop-hole, at the north end of room B, effectively covers the entrance from the outer stable to the ditch.

[15] In the absence of any dated tomb-groups or stratified series the coarse wares of Roman Cyrenaica remain relatively uninformative. Mr David Smith, of Durham University, is studying the sherds collected during the Libyan Map expeditions of 1950–1.

[16] One pencilled graffito gives the name of Oberleutnant Friederich Von Freiharth and the date 1916. This German Officer must have been one of the enemy agents landed by submarine during the Great War in order to stir up the Senussi against the Italian and British allies.

[17] The possibility of these being secondary must be borne in mind. See above, p. 133.

a Roman date. Rohlfs' (and Bates') hypothesis of an early Libyan origin was based on the so-called ' polygonal ' masonry of the berm revetment, and the elaborate rock-cutting of ditch and basements. Neither of these considerations has any validity : irregular masonry is frequently found in Libya in revetment walls of the Roman period ; rock-cutting was prevalent in Cyrenaica in both the Greek and the Roman periods[18]. Fortified buildings with vertical-sided rock-cut ditches are quite often encountered in the Cyrenaican Gebel, and there is no reason to doubt that most of them belong to the Roman period. At Ain Mara, between Cyrene and Derna, there is, for example, a fort of typical Roman masonry which is surrounded by a rock-cut ditch, with chambers in its outer face, exactly as found at Gasr el-Heneia[19]. The basements of Heneia are, however, apparently unique.

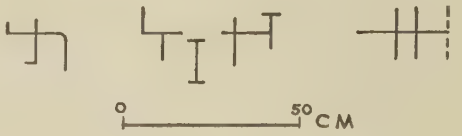


Fig. 6. DETAIL OF GRAFFITI

In considering the probable date of Heneia we must take into account its strategic rôle. The site itself has little local significance : it is in open desert and offers neither water nor agricultural facilities. Its importance lies in the fact that it controls the southern approaches to Agedabia, and sits astride one of the main caravan routes from the distant oases of Augila. In antiquity, as to-day, Agedabia was a key point on the caravan route from the coastal plains of Cyrenaica to the oases of Augila and Gialo. Inscriptions published by Ferri in 1926 show that Agedabia (which can be identified with the *Corniclanum* of the Peutinger Map) had a Roman garrison between A.D. 15 and 51 : several of the soldiers who carved their names on the rocky outcrop near the modern village were of Syrian origin[20]. The fort which they garrisoned has not been positively identified, but probably lies beneath the ruins of an early Islamic fortress which has, in turn, been enclosed within the walls of an Italian ' fortino[21] '.

In these circumstances we need have little hesitation in interpreting Gasr el-Heneia as a frontier outpost, probably of the 1st century A.D., dependent on the Roman fort at Agedabia. How long it retained a garrison we cannot judge, but it may well have been brought back into use under Justinian, when the Syrtic *limes* was re-organised, and the city of *Boreum* (Bu Grada, near Marsa Brega) fortified[22]. Although its defensive arrangements are not of the pattern encountered in the northern *limites* of the Roman

[18] Not only the Greek tombs of Cyrene, but also the Roman tombs of Tocra, are almost entirely rock-cut. This rock-cutting tradition seems to have died out during the later Roman period and has not yet been found in a Byzantine context.

[19] The Ain Mara Fort, which is evidently the one referred to by Synesius in his description of *Hydrax* (Letter 67) will be published elsewhere.

[20] S. Ferri, ' Firme di legionari della Siria nella Gran Sirte ', *Rivista di Tripolitania* II (1926) 363–86. (= *S.E.G.*, IX, 773–95).

[21] The so-called ' Christian basilica ' of Agedabia (Romanelli, *La Cirenaica Romana* (Verbania, 1943), fig. 31) is actually the only surviving fragment of the great early Islamic fortress-palace seen at Agedabia by Pacho and other 19th-century travellers.

[22] Procopius, *De aedificiis*, VI, 2. For *Boreum* see *Journ. Rom. Stud.*, XLI, (1951) (forthcoming).

PLATE I

a. GASR EL-HENEIA. VIEW FROM OUTER EDGE OF NORTH DITCH, SHOWING INNER FACE OF DITCH, REVETMENT OF BERM AND OUTER WALL OF FORT

b. GASR EL-HENEIA. ROCK-CUT BASEMENT. CENTRAL BAY OF GALLERY B

facing p. 140

c. GASR EL-HENEIA. THE STAIRCASE WELL LOOKING SOUTH

d. GASR EL-HENEIA. RECESSES FOR HORIZONTAL BEAMS OF BRIDGE OVER SOUTH DITCH, AND FOR VOUSSOIRS OF SUPPORTING ARCHES

e. GASR EL-ATARESH (GHEMINES). GENERAL VIEW FROM NORTH-EAST

f. GASR EL-CHEL (GHEMINES). GENERAL VIEW FROM SOUTH-EAST

g. GASR EL-ATARESH (GHEMINES). VOUSSOIRS OF DOOR-ARCH IN EAST WALL

h. GASR EL-ATARESH (GHEMINES). SOUTH WALL: DETAIL OF MASONRY

i. GASR EL-CHEL (GHEMINES). DETAIL OF MASONRY, SHOWING RE-USED ORTHOSTAT

empire, Gasr el-Heneia is in size and function a typical desert outpost of Roman Libya. The elaborate rock-cutting of ditch and basements merely reflects the influence of the Cyrenaican rock-cutting tradition on the planning of such military works.

Having disposed of Gasr el-Heneia as a possible relic of native Libyan military architecture, let us now consider the group of fortified buildings in the area of Ghemines for which Bates also claimed a Libyan origin. Of these the only published description is that given by the Beechey brothers who travelled along the Syrtic coast in 1821, and subsequently surveyed the ancient cities of the Pentapolis[23]. Unfortunately the Beecheys never published the plans which they had drawn in the Syrtica, and which would undoubtedly have been most useful, so we can only quote their description[24]. After speaking, in general terms, of the fortified buildings which they had encountered all the way along the Syrtic coast, and which they rightly attributed to the Roman period, the Beecheys go on to say: 'At Ghemines, which is a day's journey to the northward of Carcora, there are several interesting remains of ancient forts, some of which are altogether on a different plan from those [previously] described. They are built of large unequal-sized stones, put together without cement, and made to fit into one another in the manner which has been called Cyclopian. Their form is a square with the angles rounded off, and some of them are filled with earth, well-beaten down, to within six or eight feet of the top; the upper part of the wall being left as a parapet to the terrace which is formed by the earth heaped within it'. They then describe one unusual example of these forts, which had projected towers in the centre of each of its walls.

During the summer of 1950 the Libya expedition was able to work for two days in the Ghemines area, examining several of the best-preserved forts in the group to which the Beecheys refer. The fort with the projecting towers, of which Bates gives a reconstructed plan and section, was not in the group which was examined, and its site could not be identified on the ground; but it was later rediscovered, near Suani Tica, 20 km. south of Benghazi, in the course of an air reconnaissance. It is, in fact, much less 'Libyan' in appearance than the Ghemines examples and need not be considered here.

The largest and best-preserved of the Ghemines forts is called Gasr el-Ataresh by the local population and lies 2 km. NE of the centre of Ghemines village. During the Italian occupation it had been incorporated in a fort built by the Italian army, the massive walls of the ancient structure being repaired in order to support an observation tower. The Italian fort has since been entirely demolished for its stone, but the ancient structure has survived almost intact, thanks to its heavy and intractable materials. Gasr el-Ataresh measures 38 by 36 metres externally, and has corners so rounded as to give the impression that the plan is oval. Its outer walls, the only ones visible, consist of large irregular stones, roughly coursed, but with little or no attempt at facing or trimming (PLATE II, e) A barely perceptible depression, running outside these walls, marks the site of a shallow ditch, as found with the other Ghemines forts.

Although the inner arrangements of Gasr el-Ataresh are not visible, the outer wall contains, in the centre of its east face, the northern jamb and three voussoirs of the arched entrance doorway, which originally gave access to the interior (PLATE III, g). It is curious that the Beecheys failed to observe this feature, which completely destroys their strange

[23] H. W. and F. W. Beechey, *Proceedings of the expedition to explore the northern coast of Africa* (1821–22). (London, 1828) 244–6.

[24] In the Beecheys' report there are frequent references in the text to illustrations which were not actually published. It appears that H. W. Beechey proposed to publish these in a separate monograph, together with drawings made in Nubia; but this publication never saw the light of day.

hypothesis that these forts were intentionally filled with earth, and that their occupants were ' drawn up ' into the upper part of the building by ropes or similar tackle ! The internal filling of stones and earth in these Ghemines buildings is due entirely to the

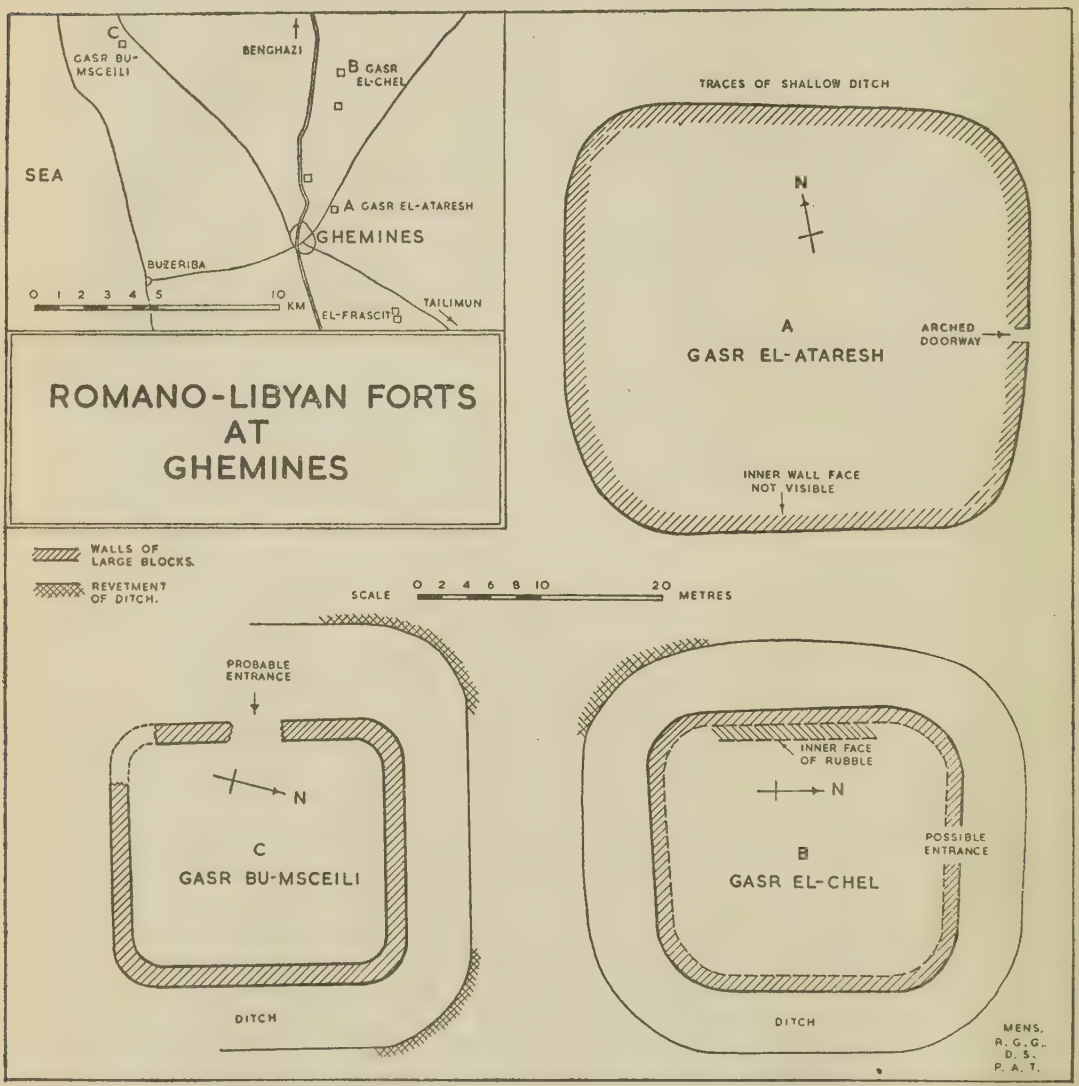


Fig. 7

collapse of upper storeys which, in some cases, may have been built of mud-brick. They must all have had, as at El-Ataresh, a single doorway at ground-level ; but these doorways are either buried beneath fallen rubble, or—more often—the wall containing the doorway, being the weakest side of the building, has collapsed.

Examination of the exposed masonry of the outer wall of Gasr el-Ataresh and the other Ghemines Forts shows the inaccuracy of the term 'Cyclopian' used to describe them. The large stones which form the outer facing (the inner facing, not visible, was probably of small materials) are not 'made to fit into one another' as the Beecheys state. On the contrary there are numerous interstices which have been filled with small stones. The arched doorway of Gasr el-Ataresh is probably sufficient refutation of the 'ancient Libyan' date attributed to these buildings; but it may be added that both at El-Ataresh, and at the nearby fort of El-Chel (PLATE IV, i), well-trimmed orthostats (probably door jambs) can be seen built into the original 'Cyclopian' masonry. Thus it must be presumed that the builders of these forts drew not only on rough untrimmed boulders, but also on the materials of pre-existing buildings, sufficiently sophisticated to have well-cut door jambs.

Of the sites examined by the expedition in 1950, some (including a pair of forts at El-Frascit, 5 km. SE of Ghemines) have collapsed into almost shapeless mounds, and are not informative without excavation. Plans of the three best-preserved (El-Ataresh, El-Chel, and Bu Msceili) are reproduced here (FIG. 7) for comparative purposes, although in no instance is any interior plan visible. In most cases these buildings are surrounded by the ruins of crude hutments of more or less contemporaneous date, which include orthostat door-jambs similar to those built into the walls of El-Ataresh and El-Chel. At El-Chel this adjoining settlement is extremely extensive, and at El-Frascit a massive rotary olive-crusher points to the agricultural character of some of these settlements. The pottery scattered among these ruins is not very informative, as much of it has been dropped by Bedouins, but there is nothing noticeably archaic, and there is a considerable proportion of recognisable Roman forms. At Bu Msceili, which lies near the sea, the main feature of note is the fact that the rough blocks of the outer walls bear the remains of a thick coating of uneven white plaster which seems, from its appearance, to be an original feature.

Although an ancient Libyan origin for these Ghemines forts is to be rejected, it must be admitted that there is a strong native influence apparent in their architecture. There are several orthodox Roman fortified buildings in this part of Cyrenaica, including a fort with angle-towers at Tailimun, some 10 km. SE of Ghemines, and a small but well-built fortified farm at Gsur el-Galida, on the roadside 17 km. south of Benghazi. These Roman structures are, like Gasr el-Heneia, well-built of dressed and coursed masonry, very different from that encountered at Ghemines. The most probable interpretation of the Ghemines structures is that they are native buildings of the Roman period, influenced by the architecture of the fortified farms, but inferior in materials and constructural technique. This inferiority is reflected in the rounded corners, which were built thus not from choice, but because a right-angled corner, dry-built of such crude materials, would soon collapse.

If we accept the suggestion that these forts belong to the Roman (or Byzantine) period, we have still to ascertain the actual century in which they were built; and, without excavation, it is difficult even to make a guess. The history of settlement in the coastal plains between Benghazi and Agedabia is most obscure, the evidence produced by the Italian excavations in the Gebel regions having no bearing on the problem. There are only two firm chronological pointers—the epigraphic evidence of a Roman garrison at Agedabia in the 1st century A.D., and the historical record of Justinian's activity at *Boreum* and in the Augila oasis. For the intervening five centuries history is silent.

These plains have, however, an importance which historians of ancient Cyrenaica have tended to overlook; they constitute the south-western *limes* zone of Cyrenaica,

which served as a cushion between the savage tribes of the Syrtic zone and the Graeco-Roman civilisation of the Cyrenaican Gebel. The historical sources which refer to the barbarian invasions of Libya during the later Empire do not reveal the origin of the invaders[25]; but there can be little doubt that the Syrtic region was the spring-board from which the barbarians launched their attacks, either westward into Tripolitania, or eastward into Cyrenaica. The fact that the Austuriani are found attacking the Tripolis in A.D. 364 and the Pentapolis some forty years later is best explained on the supposition that they were a Syrtic tribe.

In Tripolitania, as has been described elsewhere[26], a deep zone of settlements of *limitanei* was established from the 3rd century onwards, to meet the threat. It can hardly be doubted that similar measures were adopted in Cyrenaica, not in the arid zone south of the Gebel Akhdar, but along the Syrtic plain south of Benghazi which offered water resources to any invading bands. The forts at Ghemines, and the vast number of more or less similar structures scattered throughout the plain between Benghazi and Agedabia, can therefore be interpreted as representing settlements of *limitanei* in rear of the military outposts of the Agedabia area. That these Cyrenaican *limitanei* were much more backward than their opposite numbers in Tripolitania is evident from the character of their forts, and of the settlements adjoining them. They were recruited, no doubt, from the more docile Libyan tribes of Cyrenaica, from those who in Italian colonial terminology would have been called the 'sottomessi'—the submitted[27].

The purpose of these settlements was to block the main route from the bottom of the Syrtic gulf to the coastal cities of the Pentapolis. The policy of playing off one barbarian tribe against another, of using 'foederati' to defend areas which had no adequate military garrison was, of course, a common feature of later Roman history. In some cases the policy succeeded, but there was also a danger that the 'foederati' might throw in their lot with the invaders, or, at the best, adopt a neutral attitude. That such actually happened in Cyrenaica can perhaps be inferred from a passage in a letter of Synesius, in which it appears that a 'half-barbarous' tribe called the *Macetae* informed the barbarians of the defenceless condition of the province, whereupon the latter 'came countless like the leaves and the flowers in spring'[28].

If the Ghemines forts, and the others of this type in the coastal plain, can be proved by excavation to belong to the latter part of the 4th century A.D., then there will be good grounds for interpreting them as constructed by the *Macetae* under Roman direction. If they belong to an even later period, they may result from the policy of Justinian who, as Procopius attests, took energetic steps to establish security in south-west Cyrenaica. Whatever their date may be, these forts seem to mark the impact of Roman policy on a primitive society whose only spontaneous architecture is represented by the crude huts which are to be found scattered in many parts of Cyrenaica[29]. The native strongholds to which Diodorus refers have still to be identified.

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[25] Ammianus, XXVIII, 6; Synesius, *Catastasis* and *Letters* (*passim*).

[26] *Journ. Rom. Stud.* XXXIX (1949), 81–95, and XL (1950), 30–8.

[27] The equivalent Latin term would probably be *pacati* (cf. Procopius, *De aedif.* VI, 3) or *feoderati*.

[28] Synesius, *Letter* 130. (ed. Fitzgerald).

[29] During the 1950 expedition native sites, with few traces of romanisation, were found between el-Abiar and Charruba, and also near Martuba, southeast of Derna. The later prehistory of Libya has been sadly neglected, and merits investigation.

Prehistoric America: comments on some C14 dates

by G. H. S. Bushnell

ANTIQUITY has recently received some information* of great value in connexion with radiocarbon dating from the advisory committee on the subject set up by the American Anthropological Association and the Geological Society of America. It consists of an historical foreword, a corrected list of dates, a number of reports on specific problems, a summary and a bibliography, partly in typescript and partly on microfilm. The list differs little from the printed one which was distributed to a number of interested people in September 1950, though it contains a few additions and corrections, and the real importance of the material now received lies in the reports and the summary.

The summary includes a discussion of the degree of accuracy which can be expected of the measurements taken hitherto, and of the nature of the errors which may arise. A perusal of this shows how necessary it is, since attempts to use the method to give dates to a greater degree of accuracy than the measurements justify are bound to bring undeserved discredit on it. It must be said at the outset that the Committee is convinced that the method is sound in principle. Certain serious difficulties and inconsistencies in dating remain, as will be seen in the discussion of individual reports, but there is an impressive and growing number of dates which are acceptable and mutually consistent, and there is little doubt that further measurements will eliminate most of the problems. Cases have already occurred in which unlikely results have been tracked down to such sources as misinterpretation of field evidence or confusion of samples in the laboratory.

Of the eleven reports by specialists on particular problems, eight are concerned wholly with America, and comments on them will be made below. The remainder, on Pollen Chronology, the Lascaux Cave, and Dates in the Near and Middle East, must be left to a later date and a hand more competent to deal with them. Before proceeding to a discussion of the American results, however, a few words must be said about the general principles touched on in the summary. The basis of the method is fairly widely understood, so it will suffice to say that all living things contain a constant amount of a radioactive variety of carbon, which starts to diminish when the organism dies. Half of it will disappear in about 5568 years, half the remainder in the next 5568 years, and so on, and the quantity present will give an indication of how long ago death occurred, up to a limit, with present methods, of 19,000 years. The usual way of determining this quantity is by measuring the radiations from a prepared sample in a delicate counting instrument,

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* We wish to thank Mr Frederick Johnson, of the Robert S. Peabody Foundation for Archaeology, Andover, Massachusetts, for sending a copy of the manuscript of 'Radiocarbon Dating', which was published as Memoir no. 8, *American Antiquity*. vol. 17, no. 1, part 2. We hope to publish further articles dealing with dates obtained by the radiocarbon method for the Stone Ages and for the early phases of history and prehistory in the Middle East. Ed.

with which the degree of accuracy obtained depends on the length of time given to the count. The results are given in the form of a figure denoting the age in years, plus or minus a smaller figure denoting the likely error in measurement, technically called the random error. For example, the age of a sample from the earliest human level in South Chile is given as 8639 ± 450 years, which means to say that there are two chances in three that the correct age will be within 450 years, either way, of 8639, and nineteen chances in twenty that it will be within 900 years of it. The figures given are the result of 48 hours of continuous counting and all are subject to errors of the same order of magnitude ; in order to halve the error, the time of counting would have to be multiplied by four, and considerations of cost have precluded the attainment of greater accuracy on these lines. It follows that the method is, in its present degree of refinement, more useful in giving an idea of the age of ancient, undated cultures than in assigning precise dates to more recent ones, whose approximate age is already known. It is, indeed, quite possible that when two cultures differ little in age, the figures obtained may appear to reverse their order owing to random error, whereas a longer count would show them in their true sequence. There is little doubt that means will be devised to reduce these statistical errors, and when this has been done the usefulness of the method will be much increased.

Other important points discussed are the choice of samples, and precautions against contamination. Most organic matter is suitable, but the quantity required depends on the concentration of carbon in it, and, for example, unburnt bone is particularly liable to contamination after death and so is generally unreliable. It is also important to ensure that the sample is really contemporary with the culture which it is sought to date ; an instance occurred in Peru in which a digging stick, embedded in the wall of a house, appears to have been cut many years before the house was built. Museum samples may have had preservative applied, which will vitiate the measurement completely, and cotton wool or shavings used for wrapping may adhere to a sample and produce a similar effect. In general, the choice and collection of samples requires a good deal of care and common sense.

The development of the method and the figures supplied are due to Drs W. F. Libby and J. Arnold of the University of Chicago, and the advisory committee consists of three archaeologists and a geologist. The summary is the work of the committee, and they conclude it by stressing the importance of collaboration between the different sciences concerned, if the usefulness of the method is to be maintained and extended.

The appearance of man in the New World is bound up with the advance and recession of the Pleistocene ice-sheets so the report on geological dates by Prof. Flint of Yale, a member of the Committee, is relevant here. The present limit of 19,000 years has made it impossible to date anything earlier than the warmer period between the two last advances of the final or Wisconsin glaciation ; this warm interval proves to be approximately 11,000 years old, about half the previous estimates. This result was obtained from the Two Creeks Forest Bed in Wisconsin, and it seems to agree with the European Alleröd peat. If further work supports this agreement, wide possibilities of climatic correlation between Europe and North America will be opened up. The final or Mankato glacial advance, whose end moraine extends across country for hundreds of miles and is a very useful marker, is estimated to have occurred only a few hundred years later. Some later geological samples serve to date the draining of certain lakes, some of which are associated with human occupation.

My comments on the age of early man in America are based on various reports on particular regions as well as on a general one by F. H. H. Roberts. Briefly it can be said that the various early hunting cultures, characterised by the use of rather large, well-made

stone points and the spear-thrower, and association with extinct animals such as the ground sloth, the mammoth, the indigenous horse and a species of bison, existed from about 10,000 to 7000 years ago. Most of the evidence comes from the United States, but one of the most interesting results is the age of about 8500 years obtained for the earliest occupation in southern Patagonia, considerably greater than the 5000 originally estimated by Junius Bird, the finder. Of the dates from the United States, Folsom is still the earliest which can be relied on, at about 10000 years (8000 B.C.). that is to say 1000 years or less after the Mankato glacial maximum, but we still have no idea of its range in time and it almost certainly overlaps similar but generally younger cultures using different types of point. The age of about 4300 years, which seemed far too young when published last year, has since proved to come from a hearth in a later channel cutting through Folsom deposits. A sample from one of the other hunting cultures at Lime Creek, Nebraska, gives an age of about 9500 years. This is associated with the Scottsbluff point, one of the several distinct types formerly called Yuma, which is also dated rather later than 7000 years by a sample from Sage Greek, Wyoming, so it has a range of at least 2500 years. A number of other samples fall within this age-span, some of them from open camp sites but three come from caves of rock shelters. Several pairs of woven rope sandals, found under a bed of pumice in a cave in Oregon, give an age of about 9000 years. The Leonard Rock Shelter in Nevada has supplied some wooden dart foreshafts giving an age of about 7000 years, and some dart shafts from Gypsum Cave in the same State may be older, but their exact relationship to the sloth dung from the cave, which gave dates of from 8500 to 10500 years, is uncertain, and they themselves were not tested. A few samples of great age from Mexico have been measured. For our present purpose, the only one which needs comment is a peat from the horizon in which lay the bones of Tepexpan man, near Mexico City. This confirms de Terra's estimate of the age of this horizon as 11000 to 12000 years, but it must also be said that some anthropologists believe that the skeleton was intruded into the stratum at a later date.

There is considerable variety in the artifacts associated with the hunting cultures in the 10,000 to 7000 year span, and this, together with the early date of the population of South Chile, indicates either that man arrived in America before the Mankato glacial maximum or that there had already been a number of independent migrations by about 8000 B.C. or, more probably, both. An age determination of the pre-Folsom culture of the Sandía cave in New Mexico might prove illuminating in this connexion.

It has generally been held that the earliest food-gathering cultures of North America were contemporary with the hunting ones already mentioned, but radiocarbon dates suggest, on the contrary, that they succeeded them. The three stages of the well-known Cochise Culture of Arizona and New Mexico have each yielded a date, the oldest being about 6300 years and the youngest 2500, so it succeeds the latest dates we have for the hunting stage with a short gap, and it lasts from before 4000 B.C. to at least 500 B.C. This brings it very close to the beginning of the Basket Maker culture of the Southwest, after which the sequence is securely dated by tree rings. In the eastern and north-eastern United States, ' Archaic ' cultures, in which food gathering played a part sufficiently important to allow the establishment of permanent settlements, have been found to fall within the date range of the Cochise. The Lamoka of New York begins about 3500 B.C., and the Annis shell midden of Kentucky was occupied by 3000 B.C.

At a later stage, Bat Cave in New Mexico has supplied important evidence for the early introduction of maize, since it contains a stratigraphical series ranging from primitive to modern types of cob, of which the earliest is contemporary with the middle stage of the Cochise Culture. The earliest dated sample, of charcoal, from the cave is

about 4000 B.C., and the next about 900 B.C. The report on the subject is not clear, but it appears that the earliest maize occurs with the second sample. This indicates that agriculture began a good deal earlier than had been believed, but it is supported by evidence from Mexico which will be mentioned in its place.

One of the greatest difficulties the method has raised is in connexion with the succession in the Ohio region and the Mississippi valley. It has hitherto been believed that the Hopewell, one of the most striking mound-building cultures, was preceded by a less-developed one, the Adena. There is almost conclusive indirect evidence for this in Ohio, the type area, though the only place where they have been found in superposition is in the marginal Lower Mississippi area. The radiocarbon dates show a reversal of this sequence in Ohio, giving a range of about 400 to 1 B.C. for Hopewell, and A.D. 450 to 800 for Adena. In the Lower Mississippi valley, the Hopewell dates are much later, A.D. 700 to 800, and the Adena ones range from about A.D. 700 to 1300. J. B. Griffin, who examines the evidence, concludes that most of the samples tested are unsatisfactory for various reasons, and considers that the question is still open. More measurements are urgently required.

Very interesting results come from the early agricultural settlements of the Valley of Mexico, the Middle Cultures of Vaillant. The earlier stage is dated about 1500 B.C. at Zacatenco, instead of the first few centuries A.D. as had been estimated, and it is most probable that agriculture was introduced long before. The later stage, for which two consistent readings are available, is dated about 500 B.C.; one of them is from under the lava flow near the Cuicuilco pyramid, well known to readers of Vaillant's 'Aztecs of Mexico'. There are also readings from the early stages of the Zapotec culture of Monte Albán in Oaxaca, which support the correlations with the Middle Cultures of the Valley of Mexico already made by Prof. Alfonso Caso.

The South American measurements come from two regions, South Chile which has already been mentioned, and Peru. Junius Bird comments on them in the most detailed report in the series; the omission of two charts makes some of the details difficult to follow, but Bird's field experience renders his comments very useful. The date of about 800 B.C. for the base of the Cupisnique horizon in the north coast of Peru has been known for some years (see my letter in ANTIQUITY no. 92), but it is now possible to go considerably further back, and it is known that the Preceramic agricultural settlement dates from at least 2400 B.C. in the Chicama Valley. This is not the base of the deposit, and extrapolation indicates that it goes back about 300 years more at the site studied. One of the Preceramic dates (No. 318) published in September 1950, which did not fit the rest of the series, has fallen into line on remeasurement. Two samples higher in the North Coast series are dated very considerably later than was expected. The beginning of the Gallinazo Culture of the Virú Valley is dated at about 500 B.C. instead of A.D. 300 as had been estimated. A sample from near the beginning of the important Mochica Culture (formerly called Proto-Chimú) at its type site at Moche seems to indicate that it began between 300 and 400 B.C. Since the Gallinazo Culture in the neighbouring Virú Valley was displaced by the Mochica in its later stages the two results are not inconsistent, but the Mochica sample differs to such an extent from estimates made on other grounds that it cannot be accepted by itself. Previous estimates, based on consideration of the succession as a whole, had placed the Mochica between A.D. 600 and 1000, and a recent study of rate of accumulation of debris puts the end of it as late as A.D. 1150. If Gallinazo and Mochica are as early as the radiocarbon measurements show, it will be difficult to space out later cultures to fill the time interval before the Inca expansion. More readings are needed and should not be difficult to obtain.

In the South Coast a test on some mummy wrappings from Parácas Necropolis gives a date of about 300 B.C. This again, is, earlier than was expected but Bird does not see why it should not be true. This cemetery has been correlated with the Mochica in a widespread Mastercraftsman Period, on typological grounds, so the agreement between their radiocarbon dates does not lack support. Two tests were made on material from Nazca, supposed to be of much the same age as Parácas Necropolis, but the results are not consistent enough to make useful comment possible.

These remarks by no means exhaust the information to be derived from the measurements available, but they cover the matters of greatest general interest. Results which, for one reason or another, seem to be in error, are bound to occupy a disproportionate amount of space, but it must be emphasised that the majority are mutually consistent and are of great help in building up a coherent picture of America's past.

NOTE. For further information on the subject see 'Archaeological Dating by Radioactive Carbon', by F. E. Zeuner, *Science Progress*, April 1951, 225–38, with bibliography. ED.

Important New Books and Articles

The inclusion of a book in this list does not preclude its subsequent review

The Lost Villages of Medieval England, by M. W. Beresford. *Geogr. Journal*, June, 1951, 129–49. [A fuller account, with air-photos. and maps, of the subject dealt with in Antiquity, No. 95].

Iron Age Cultures in Southern Rhodesia, by Roger Summers. *S. African Journ. of Science*, Nov. 1950, 95–107.

A Note on the Pre-Bantu Inhabitants of N. Rhodesia and Nyasaland, by J. Desmond Clark. *Ibid.*, Oct. 1950, 80–85.

Monuments Funeraires Pre-Islamiques de L'Afrique du Nord, by Maurice Reygasse. 130 pp. 174 collotype illus., map. Paris : Arts et Metiers graphiques. [Deals with the tumuli, stone circles, ' dolmens ', artificial burial-caves, etc., of N. Africa].

III Sprawozdanie (3rd report) z Prac Wykopaliskowych w Grodzie Kultury Luzyckiej w Biskupine w Powiecie Zninskim, by Jozefa Kostrzewskiego. Poznan, 1950. 373 pp., many illustrations and maps. [This is a Report on the excavations at Biskupin in 1938–9 and 1946–8; the French resumé makes it comprehensible to non-Polish readers. B. is an outstanding site and its excavation has yielded remarkable results bearing on the social structure and skill of the late Bronze Age population of Poland whose significance is not yet appreciated by prehistorians. Unfortunately the quality of the paper and consequently of the illustrations also is very poor. An article on B. was published in Antiquity, No. 47, 1938].

Prehistoric Migrations in Europe, by V. Gordon Childe. H. Aschehoug and Co. (W. Nygaard), Oslo. N. Rr. 35. 250 pp. 229 illus.

Prehistoric and Roman Monuments in England and Wales, by Jacquetta Hawkes. Chatto and Windus, London. 18*s*. [This super-guidebook is so good that we cannot recommend it too highly, and propose to review it later. *Buy it now*].

Social Evolution, by V. Gordon Childe. Watts and Co., London, 10*s*. 6*d*. [An attempt to examine ' the theory of social evolution, deduced by Herbert Spencer and Lewis H. Morgan from the comparative study of existing societies, . . . in the light of the science (archaeology) which presents societies in a chronological sequence. The results, though ' certainly unfavourable to any theory of unilinear evolution ', are also quite certainly ' interesting and stimulating to the reader ', especially the Conclusions in Ch. xii].

N.B. These lists are mainly compiled from publications sent to us for review ; they are necessarily selective.

Notes and News

DRIFT TIMBER IN THE WEST

When Pennant was in Islay in 1772 he records : ' I was also favoured with several of the nuts, commonly called *Molucca* beans, which are frequently found on the western shores of this and others of the *Hebrides*. They are the seeds of the *Dolichos urens*, *Guilandina Bonduc. G. Bonducetta*, and *mimosa scandens of Linnaeus*, natives of *Jamaica*. The fifth is a seed called by Bauhin *fructus exot* : *orbicularis sulcis nervisque quatuor*, whose place is unknown. The four first grow in quantities on the steep banks of the rivers of *Jamaica*, and are generally supposed to drop into the water, and to be carried into the sea : from thence by tides and currents, and the predominancy of the East wind, to be forced through the gulph of *Florida*, into the *North American* ocean, in the same manner as the *Sargasso*, a plant growing on the rocks in the seas of *Jamaica*. When arrived in that part of the *Atlantic*, they fall in with the Westerly winds, which generally blow two-thirds of the year in that tract ; which may help to convey them to the shores of the *Hebrides* and *Orknies* (*Phil. Trans.* abridged, III, 540). I was for resolving this phenomenon into ship wrecks, and supposing that they might have been flung on these coasts out of some unhappy vessels : but this solution of mine is absolutely denied, from the frequency and regularity of the appearance of these seeds. *American* tortoises, or turtle, have more than once been taken alive on these coasts, tempest-driven from their warm seas ; and part of the mast of the *Tilbury* man of war, burnt at *Jamaica*, was taken up on the Western coast of *Scotland* ; facts that give probability to the first opinion '.[1] There is no mention here of the Gulf Stream ; and the record reflects the objective curiosity of the 18th century, when the explanation of natural phenomena might still be treated by gentlemen as matter of ' opinion '.

Since Pennant's day some additional facts have been recorded. The occurrence on our western shores of Caribbean shells has often been noticed. In 1867 Captain Otter reported coconuts thrown up on the west coasts of Uist, and in 1911 Dr Erskine Beveridge added bamboos and confirmed turtle.[2] In 1930 Dr H. H. Thomas made a petrological examination of pumice from a chamber tomb in southwest Skye and showed that it was not of Mediterranean type ; while a source in Iceland was not excluded, the known direction of the currents led him to conclude that it was of West Indian origin.[3] Pumice is common on west Scottish sites and occurs at least once in Ireland and once in Scilly ; the writer has been told in Skye that it still comes ashore and is picked up for cleaning the hands.[4]

Recently charcoal from a site on the west side of Barra has been identified as spruce, a tree certainly not native to Barra.[5] The charcoal represents the large part of the surviving timber of the collapsed roof of an aisled roundhouse farmstead of the 1st

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[1] *Tour in Scotland*, II, 266 (edn. of 1790).

[2] *Admiralty Sailing Directions, West Coast of Scotland*, Pt. I, 1867, 5 ; *North Uist*, 1911, 5, n.

[3] *P.S.A.S.*, LXVI, 212.

[4] *Ibid.*, LXV, 350 ; LXXXII, 29 ; *U.J.A.*, 1942, 12 (Dundrum) ; British Museum (a pendant from a chamber tomb on St. Mary's, Scilly).

[5] By Mr M. Y. Orr, Royal Botanic Garden, Edinburgh. He tells me that American and Norway spruces are not distinguishable from one another from their charcoals.

century B.C., or A.D. Import of wood from Norway at that date is very improbable, even if the formidable task could have been faced of towing timber in the North Atlantic astern of the ships then available ; and it is not much more likely that the wood came from a Norse ship wrecked on Barra. Enquiries were therefore made into the probability that the spruce was drift-timber from America, and Dr J. B. Tait of the Marine Laboratory, Aberdeen, considers that the probability is high.

Dr Tait calls attention to experiments carried out in 1932, 1933 and 1934 by the Division of Fishery Research of the Newfoundland Department of Natural Resources with drift-bottles designed to float just beneath the sea-surface, out of range of direct wind action independent of water movement. Of a number of bottles set adrift in these years off the Labrador and Newfoundland coasts, and southward to the southern edge of the Grand Bank of Newfoundland in lat. 44°N., twenty were retrieved at points stretching from the Azores to Norway, with concentrations of six on the Irish coast and six on the west coasts of Scotland, including the Orkneys and Shetlands. The periods varied from 11 to 17 months, and Dr Tait thinks that spruce logs, being almost certainly subject to direct wind action, would come across more quickly. In more recent years large numbers of bottles have been thrown overboard on passage from the Clyde to the Ocean Weather Station in lat. 60°N, long. 20°W (some 200 miles south of Iceland) ; from practically all points of liberation these bottles have fetched up on the shores of the Outer Hebrides or on those of the Orkneys, Shetlands or Norway.

Mr Angus Graham tells the writer that there are still immense quantities of spruce in Newfoundland and Labrador, for example on the Hamilton River ; and that, in his experience, there should be no significant loss of trees by water-logging and sinkage in passages across the Atlantic of the periods mentioned above. Before New Brunswick and the North-eastern States were colonised, many more trees must have reached the Atlantic down such rivers as the St. John, the Kennebec, the Androscogain, the Connecticut and the Hudson. There is thus ample reason to suppose this unrequited American export capable of constituting a substantial factor in the economy of those western coastlands of the British Isles that lacked good building timber ; and the Barra farmhouse shows that use was in fact made of this drift-material at the beginning of the present era. Earlier evidence is forthcoming from Stanydale, near the west coast of Mainland in the Shetlands, where the postholes of a large building contained remains of posts up to 10 ins. diameter which have been identified to be of spruce, and also sherds of beaker, including corded B beaker.[6]

This discussion of the drift from the more northerly latitudes, where spruce grows, should not obscure the possibility of drift-timber reaching our coasts from more southerly sources, such as the Caribbean. The Hydrographer of the Navy has been good enough to confirm that the transport of the mast from Jamaica mentioned by Pennant could undoubtedly take place as a result of the water circulation of the North Atlantic, and Dr Tait quotes the record of a bottle set adrift off Puerto Rico in the West Indies which arrived 14 months later in the far north of Norway.

Judging by uprooted trees, the quantity of drift timber coming ashore is still significant, at least on the coast of Skye and the Outer Hebrides ; when America was a primeval forest the quantity may have been much greater. The distribution along the coasts is uneven, but, choosing your weather, the coastwise towage of logs can never have presented much difficulty. Nevertheless, in the two cases so far identified the drift timber

[6] *Arch. Newsletter*, Jan. 1951, 111. The source of the timber is there assumed to be Scandinavia, but the evidence now available makes this unlikely.

was used only for roof structures, and it may be doubted whether the quantity ever sufficed for building entirely in wood. It is generally true that, when wood is available, people will use it for building in preference to stone, and it is significant that the colonists who built in stone the walls of the houses of Skara Brae and Rinyo have not been identified by stone-built houses further south, while at Rothesay in Bute they are known to have used wood. In the 1st century B.C., when the timber supply was much worsened, even for fuel, the aisled round-house colonists built regularly in stone at least as far south as Tiree.

We are fortunate in knowing the significance in human terms of a really good tree coming ashore from the record of the settlement of the far northwest of Iceland in the 10th century: 'Hallstein, the son of Thorolf Mostbeard, settled Codfirth, and abode at Hallstein's-ness; he made a blood offering to the end that Thor would send him high-seat posts; after that a tree drifted aland sixty-three ells long, with a span of two fathoms in thickness. This was used for high-seat posts, and out of it are made high-seat posts wellnigh at every homestead throughout the cross-firths; the ness where the tree came ashore is now called Spruceness'.[7] LINDSAY SCOTT.

EXCAVATIONS IN BORNEO

The following note has been received from Mr Tom Harrisson, Curator of the Sarawak Museum:

Until 1949 no systematic excavations appear to have been undertaken anywhere in Borneo, although a number of interesting finds, especially of Gold and Hindu objects (reviewed by me in *Journal Malayan Branch, Royal Asiatic Soc.*, XXII, 1949, pp. 34–110) have been made more or less casually in all four of the political territories in the great island.

In 1949 (the only Museum in Borneo), we excavated some slabgraves and a large dolmen in the uplands of the far interior, finding early Chinese and Siamese pottery with ancient beads. This upland megalithic culture appears to date back not more than 800 years locally. So far it has not been possible to revisit this area and carry out more intensive research, which is planned for 1951. But during 1950 a programme of excavation was commenced in the lowlands of south-west Sarawak. Two sites were selected, already known as promising from previous casual finds. One of these is in the delta at the mouth of the Sarawak river at Santubong, which has the only carved figure in stone so far known from the lowlands, of unknown origin (see sketch in H. Ling Roth, *The Natives of Sarawak and British North Borneo*. London, 1896, II, p. 280); photograph in Harrisson, *op. cit.*, plate 14).

In the first place a ground map was made of the distribution of surface remains, including the very large quantities of iron slag, which extend over a distance of more than a mile. From this basis, six trial trenches have been dug to rock bottom. They reveal an extraordinary quantity of iron and crucibles, but so far no manufactured objects of iron. A number of well-shaped Hindu *linggam* are present among the iron, and a variety of stone pounders. Almost as numerous in quantity as the slag, are broken pieces of celadon—mainly crude export types of Ming and late Sung style, probably from the mainland south of China. Mixed with these, but mainly centred in lower levels, are crude earthenware potsherds, with patterns impressed by wooden beaters. Locally available funds are very limited, but it is hoped next year to excavate out a considerable area at Santubong, which was evidently a trading base and an entrepot of much local

[7] *Landnamabok*, trans. Ellwood, bk. II, ch. XXIII.

importance, especially in the 13th to 15th centuries. Other iron slag sites have also been located along this coast.

A rather simpler problem, because more localised, has been the excavation of a number of limestone caves in the Bau district, in the head-waters of the Sarawak river. So far, two caves have been excavated out completely and 20 others (in an area of 200 square miles) reconnoitred. The outstanding characteristics of every cave visited—even those perched on barely accessible hill-tops with very stiff rock climbs of 150-200 ft.—is the accumulation of one species of purely riverine shell, *Thiara variabilis*. Deposits of these reach up to 10 ft. thick and in many caves the number of shells runs literally into millions. In this part of the work the Sarawak Museum cooperated with the Raffles Museum, Singapore, whose Director, Mr M. W. F. Tweedie, spent a month jointly conducting the excavations of Bungoh and Lobang Angin caves, assisted by a grant from the Carnegie Foundation. A joint report on Bungoh will shortly published in the Journal of the Polynesian Society.

Considerable quantities of the same type of crude earthenware found at Santubong occur in the upper layers (only) of the Bau cave deposits. In this respect the Borneo position is parallel with the earthenware and shell-heap association previously discovered in other parts of South-east Asia, and especially in the Malay Peninsula. There is, however, one striking difference. So far, both in the two full excavations and the other reconnaisance and trial trenches, not a single proved artifact of extraneous stone has been discovered—whereas these are abundant in other South-east Asian countries. Yet the deposits are so extensive in distribution and in depth, that they indicate a long period of occupation. It seems possible that the so-called ' cave dwellers ', previously believed to be purely such, in West Borneo at least may in fact have inhabited the caves for quite short periods, living also at these times on the hundreds of thousands of swifts and bats which inhabit these caves—and which form practically the only food animal bones found in the cave habitation deposits. Since such deposits have been widely regarded elsewhere as ' mesolithic ', this implication (if supported by further research) may link in an interesting way to the results of Russian studies, recently described by Prof. Gordon Childe (ANTIQUITY, 93, 1950, p. 4). It is, no doubt, merely coincidence that Childe refers to those pleistocene dwellings as ' comparable to the long houses of the Iroquois or the Dayaks ' !

It is also possible that use may have been made in Bau sites of the hard limestone rocks smashed against the cave wall to produce a variety of splinters, to be used as required, rather than any special prepared tools !

There is no general shortage of stone implements in West Borneo. Until recently only about 50 were known from the whole island. But since 1947 the Sarawak Museum has collected more than 400, with a very wide range of types (see H. D. Collings, *Sarawak Museum Journal*, v. 1949, pp. 14–22, for preliminary notes on some types). Of these the great bulk are covered by the category ' neolithic ' as used in South East Asia. But they show a remarkable variety such as has not previously been described from the area and including axes, shouldered and crescentic adzes, beautiful black and white picks of andesite and calcite, scrapers, and what appear to be small knives not unlike but much more delicate than the Tembeling knives found in neolithic sites in Malaya. So far, no implement which can be attributed to palaeolithic times has been found in Borneo. There are two doubtful mesoliths.

In addition, in recesses of Bau caves, a number of mineralised human bones have been found. These are being submitted from the Sarawak Museum to various authorities. In view of the undoubted fact that Borneo was part of ' Sundaland ' until *after*

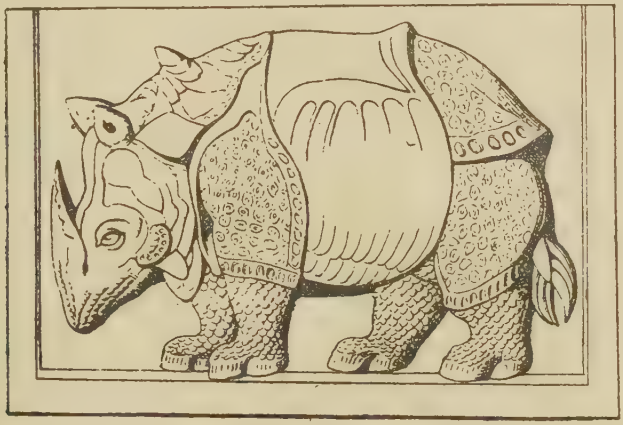
the period of the various forms of Java fossil lominids, the possibility of significant Borneo finds of this type cannot be ignored.

In Malaya, Java, Sumatra, Indo-China and elsewhere archaeological work is now in eclipse.

Sarawak is one of the very few countries in South-east Asia where it is still safe to excavate in outlying places, and where native co-operation is generous and consistent.

THE CLASSICAL RHINOCEROS AGAIN

In an article on the Classical Rhinoceros which appeared in ANTIQUITY, Vol. XXIV, June 1950, I referred on p. 67 (note 16) to a marble relief ' from Pompeii ' figured in *Die Antike Tierwelt* by Otto Keller (Leipsic, 1909, Vol. I, p. 387–8, fig. 135), showing an Indian rhinoceros. Keller gives in two passages the provenance as Pompeii and although



'POMPEJANISCHES RELIEF'

the drawing did not look at all like an antique I accepted his authority. I now find that this relief, which is, as Keller says, in the Naples Museum, is recognised as being a copy of Durer's well-known engraving of an Indian rhinoceros, done in 1515, and reproduced above. So far as I can learn, a Pompeian origin has never been claimed for it by anybody except Otto Keller. With this relief disappears the only shadow of concrete evidence that the Indian rhinoceros was known in Italy. W. F. GOWERS.

THE ORIGIN OF MAIZE*

The internationality of knowledge is exemplified by Mlle. G. Mimeur's review of the current origin-of-maize controversy (*Rev. Internatle. de Botanique Appliquée et d' Agric. Tropicale*, Vol. 29, 1949, pp. 366–377). She states the various hypotheses : maize is derived from (1) teosinte (*Euchlaena*) ; (2) a wild pod corn (*Zea mays*) ; (3) a

* Reprinted from the American *Geographical Review*, April 1951, 338–40, by permission of the American Geographical Society and the author.

hybrid between an Asiatic *Maydeae* (for example, Job's-tears, sorghum, or sugar cane) and gama grass (*Tripsacum*). She runs quickly through the early ideas to those developed by Mangelsdorf and Reeves—that teosinte is a hybrid of maize and gama grass and therefore cannot be ancestral to maize. This conclusion, she finds, has not been shaken by criticism to date.

Mlle. Mimeur summarizes the theory of pod corn as the ancestral form of maize as follows. A plant like pod corn is approximately what one would expect wild maize to have been. Pod corn crossed with *Tripsacum* might have given rise to the modern types of maize. However, we have no known wild pod corns today. Of course, wild pod corn may have become extinct, surviving only ' in solution ' in the genes of domestic corn that appear now and then to remind us of maize's former condition.

Mlle. Mimeur then turns to Anderson's observations : that maize from a remote oasis of northern Chile is like the maize of the hill tribes of Burma ; that other factors suggest very early transpacific movements of peoples ; that maize may have come from Asia to America, here to hybridize with gama grass and create teosinte and, by back crossing, the array of maize forms we see today. As Mlle. Mimeur comments, rather noncommittally, it is clear that one is dealing with a complex problem inextricably bound up with historical and geographical factors.

We can summarize her critical views somewhat as follows. Maize cannot have a Central American origin ; for the hybridization of maize and gama grass that gave rise to teosinte is apparently a recent and rapidly continuing phenomenon. Had such opportunities for crossing existed long, we should expect a more complex picture of such crosses. The picture of South American flora is still too incomplete for us to deny the possible existence of an ancestral wild pod corn east of the Andes. However, neither can Anderson's hypothesis of an Asiatic origin be rejected. Today we are less sure of the origin of maize than we were 10 or 20 years ago, but we now know more areas in which to look for useful genetic material for improving our existing strains of maize.

That this field is moving with extraordinary rapidity can be exemplified by two other recent articles. Maize from a cave in New Mexico has been reported that is estimated to date from 2500 B.C. (Paul C. Mangelsdorf and C. Earle Smith, Jr. : New Archaeological Evidence on Evolution in Maize, *Harvard Univ. Botan. Museum Leaflets*, Vol. 13, No. 8, Cambridge, Mass., 1949). It is the most primitive maize so far known. It is both pod corn and popcorn and is stated not to be derived from teosinte. Thus teosinte is again indicated to be not an ancestor, but a hybrid, of maize. Although this new find tells us much and suggests an astoundingly early date for maize agriculture in the Southwest, it still leaves the origin of maize a mystery ; it does not disprove the hypothesis of Asiatic origin, since we know that Asiatic plants were being carried to America by this time (Junius Bird : America's Oldest Farmers, *Natural History*, Vol. 57, 1948, pp. 296–303 and 334–335).

A second paper strikes at the root of the problem. In ' Maize among the Hill Peoples of Assam ' C. R. Stonor and Edgar Anderson (*Annals Missouri Botan. Garden*, Vol. 36, 1949, pp. 355–404) present linguistic, ethnologic, and botanical evidence for the antiquity of maize among the hill tribes of Assam. These tribes have a variety of native names for maize, which they use ceremonially instead of rice. They state that they have always had maize and that they had it before they had rice. Their maize is unlike the Caribbean maize introduced into Asia in post-Columbian times ; botanically, it is similar to the earliest archaeological maize of Peru. Similar maize is widespread in Inner Asia from Persia and Turkestan to Tibet and Siberia. It is maize of this type that was recorded in China as coming overland from Tibet within 80 years of the discovery of America. This

has long been pointed to as an example of the rapidity with which plants can spread. It now seems more likely to be an example either of how slowly plants spread or of the haphazard nature of literary notices of useful plants. All the evidence suggests an Asiatic homeland for maize. In America, on the other hand, we not only lack a maize ancestor and have only one closely related genus, but we continue to accumulate evidence suggesting the relative recency of maize in Middle America. Evidence from other plants, from archaeology, ethnology, and art history, suggesting very early and long-continued Asiatic-American contacts continues to accumulate. Maize may not be American!

GEORGE F. CARTER.

MR G. H. S. BUSHNELL writes :—

'Mr Carter's interesting note provokes two comments.

'First, the cave in New Mexico which he mentions is clearly Bat Cave, and the date of 2500 B.C. is a maximum geological estimate made before the radiocarbon dates were measured. F. H. H. Roberts has commented that the estimates of the age of the deposits in this cave, based on climatological studies, appear to be about twice that of the age indicated by the Carbon 14 dates. This supports my belief (see page 147 of this number) that the earliest maize sample sated by radiocarbon in this cave is about 900 B.C.

'In the second place, the startling statement that "Asiatic plants were being carried to America by this time" is not even mentioned in Junius Bird's popular article on the Preceramic horizon in Peru which is cited as the authority. From other sources, I know of two plants which may have given rise to this idea, lint-bearing cotton and a gourd. The evidence concerning cotton is genetic and a brief summary may be found in the *Smithsonian Handbook of South American Indians*, Volume 6, pages 533–538, where it is stated that there is an exceedingly strong case for the introduction of an Asiatic cotton to the New World by agricultural folk across the Pacific. The gourd is studied in a paper by T. W. Whitaker and J. B. Bird, "Identification and Significance of the Cucurbit Materials from Huaca Prieta, Peru" (*American Museum Novitates*, No. 1426, September 13, 1949). The evidence in this case boils down to a broad-seeded variety of a single species, *Lagenaria siceraria*, which is normally regarded as an Old World type. The comment the authors make is "This new information suggests that if *Lagenaria siceraria* is a native of the Old World, it was introduced into South America at a very early date. On the other hand, there is no clear evidence that negates the idea that this species was a New World endemic and was domesticated in the Americas".

'It is important to realise that both these plants are present throughout the Preceramic Horizon in the Chicama Valley in Peru, long before the introduction of maize, so, if the new radiocarbon dates are to be believed, they were present before 2400 B.C. and probably as early as 2700 B.C. If trans-Pacific voyages were possible at this date, our ideas about the history of navigation will require very radical revision'.

'BATCH' IN PLACE-NAMES

A correspondent writes :—

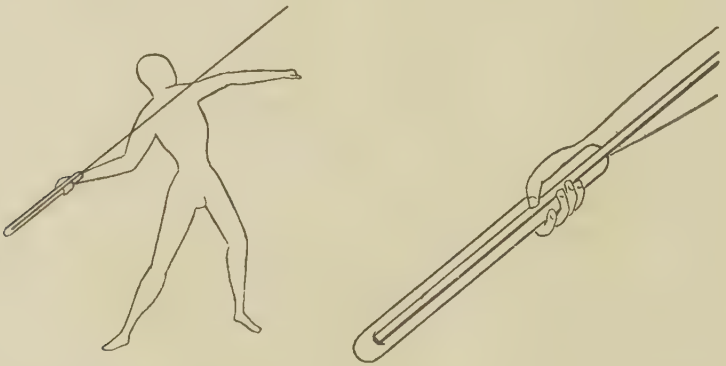
'On page 61 of ANTIQUITY, No. 98, June 1951, in your article "Place-names: a review", you refer to the word "batch" as having survived down to the time of John Aubrey. My information is that this word is still in everyday use in this part of Somerset as a descriptive noun, though its application is now probably confined to specific localities. It seems to be a North Somerset word. A lady born and brought up in South Petherton and now living in Bridgwater town told me that she did not

know it. On the other hand a lady who lives at and has had a lifelong connexion with Street, two miles from Glastonbury, said that the word was quite familiar to her (though she could not say when she had last heard it) and mentioned a specific locality in Street to which it is applied. She was, however, doubtful whether the word is now in use as a general descriptive noun. About four years ago a friend mentioned to me that she had heard the direction " down along arter the zide of thik batch " given somewhere in the neighbourhood of Mudgley Hill, on the road between Wedmore and Glastonbury'.

' A friend in this parish tells me that the word is at present applied mainly to slight rises in roads—generally to a slight rise over a rhine (drainage ditch) or water-course ; but this part of the county is almost exclusively flat moorland, and I am inclined, from what I have been told, to believe that in the more hilly portions of North Somerset the word may be applied to more substantial rises'.

POSSIBLE MAGDALENIAN SURVIVALS IN AFRICA

In my article with the above title in ANTIQUITY, 1951, pp, 19–28, photographs of Mohamed Bugar using his Sarwa spearthrower were not printed, while the Darfur copy of a Sarwa spearthrower made by Harun Yasin was depicted on plate I, fig. 2.



The Darfur copy is not an intelligent one, in that it does not lengthen the arm of the person throwing the spear, as Dr G. H. S. Bushnell has pointed out to me. The sketches shown herewith were made from the photographs omitted, and show how the Sarwa hold the spearthrower and use it. From them it is clear that the Sarwa spearthrower does lengthen the arm.

A. J. ARKELL.

Reviews

KARTHAGO: REVUE D'ARCHÉOLOGIE AFRICAINE, I, 1950. 200 *pp.*, 14 *plates.* (*Paris, Librairie Klincksieck*).

This new periodical is to be the annual publication of the *Mission Archéologique Française en Tunisie*, a recently-formed body which is at present working within the framework of the Tunisian Service des Antiquités, but which will, presumably, eventually become an autonomous body like the French archaeological institute at Beyrouth.

Although the French administrations in North Africa have pursued a liberal policy of archaeological publication, *Karthago* will be by no means redundant. There has long been a need for a periodical like *Syria* in which current archaeological research in North Africa could be published: too many articles of importance have become buried in the *Comptes-Rendus* of the Academy of Inscriptions and in other periodicals of metropolitan France.

The first issue covers a wide field, M. Gobert describes a palaeolithic deposit at Sidi Zin, near Kef, in a well-illustrated article with an important supplement (by Vaufrey) on the fauna. M. Picard, Director of Antiquities in Tunisia, discusses some colossal Victories dug up many years ago on the Byrsa hill of Carthage, and attributes them to a trophy commemorating the eastern campaigns of Lucius Verus. In the final article, M. Saumagne reconstructs, from a large number of fragments, the sense of an important fiscal document of the 4th century A.D.

These are all scholarly articles, which will appeal more to the specialist than to the general reader. It is understandable that *Karthago* should wish to open its career with a salvo from the heavy artillery. On the other hand, one cannot but feel that this new periodical will get much greater support if it can leaven its subject-matter by including articles of a more general character. The bibliography of ancient Africa is vast, diffused, and difficult of access to many students outside France. If *Karthago* can include, from time to time, summaries and bibliographies of Tunisian archaeology, and general discussions of the problems which are being investigated (and await investigation), it will be eagerly read in many parts of the world. Perhaps no ancient site has captured the public imagination more than Carthage. A slight element of that spirit of 'vulgarisation' in which French writers excel could make *Karthago* a powerful moral and financial aid to the French mission's work in Tunisia.

R. G. GOODCHILD.

ARCHEOLOGICKÉ ROZHLEDY. *Ročnik II, Sešit* 1–2, 1950, *pp.* 152. *Praha I, Břehova* 7, *Prehistorický Ustav.*

The Government of Czechoslovakia has wisely provided the State Archaeological Institute with liberal funds for the conservation of national antiquities, both prehistoric and historic, and for excavation and research. An intensive campaign of diggings, admirably planned by Dr Böhm, Director of the Institute, has brought to light each year interesting and even sensational discoveries that are relevant to the prehistory of Europe as a whole.

To make available interim reports of the results obtained, pending publication of the full reports, which must take time, the Institute founded in 1949 a well-illustrated 'Archaeological Newsletter' with Russian and French summaries of all the articles.

The first number of Volume II contains notices of several important discoveries that deserve mention here.

In the famous mammoth hunter encampment at Dolní Věstonice, Dr Klíma, who has succeeded Dr Absolom at Brno, discovered another palaeolithic burial in an area supposed to have been fully explored by his predecessor. The body, of a female, lay on her right side in the strictly contracted position, coloured red with ochre, and covered by two mammoths' shoulder blades.

In the New Stone Age, two fortified camps have been discovered in Moravia, both belonging to the Danubian II culture characterised by ' Moravian painted ware '. At Uherský Brod no trace of a rampart was discoverable, but at Hluboké Mašůvky the ditch (apparently supplemented by a palisade) was 1.8 to 2.8 m. deep, and 2.4 to 4.6 m. wide at the mouth. Judging from the section explored, the station must have had an area of 60,000 sq. m., or a little over 14 acres. The excavator, Dr Neústupný, calculates that the excavation of the ditch would today take 1600 man-days and in neolithic times, 4850.

Special attention has been quite naturally concentrated on the fortresses of the early Slavs. The application of modern technique to the excavation of these imposing ' earthworks ' has brought to light remarkably interesting details of the timber constructions which support the walls of stone or earth. The fortress of Libušín, for example, is surrounded by a gigantic rampart faced externally with a wall of quarried stone combined with transverse beams, behind which were arranged wooden chambers 2.5 by 1.4 m. square, packed with earth and stones. Some admirable photographs and reconstructions illustrate these imposing fortifications of the 10th century and the very amusing *graffiti* found on some of the stones from the ramparts. No less impressive and really more difficult to obtain were the results at Stará Kouřim, where the ramparts were composed entirely of earth and beams, without masonry. V. G. CHILDE.

NEAPOLIS TREASURY TABLETS. *By* GEORGE C. CAMERON. *Oriental Institute Publications, Vol.* LXV. *Univ. of Chicago Press*, 1948 : XVIII, 214 *pages* ; 46 *plates*. $12.00.

Down to the present time the Elamite language of S.W. Iran has continued to be the step-child of cuneiform studies, mastered by only a few scholars such as Scheil, Weissbach, Hüsing, Bork and F. W. König. This is only natural for the linguistic material was poor and monotonous in character, consisting chiefly of inscriptions of the Elamite kings found at Susa of the 13th and 12th centuries B.C., and of late Elamite versions of Achaemenian inscriptions. Quite recently this material has been considerably increased by the excavations at Persepolis of the Oriental Institute of Chicago University (1933–4). In the town wall Herzfeld found about 7000 clay tablets dealing with economic matters, dating from 510 to 494 B.C. and still unpublished. In 1936–8 Schmidt found in the Treasury about 750 similar business-texts of 492–460 B.C., and of these those considered to be the most instructive—84 in all—are published in the present volume, together with one in the Akkadian language.

These texts are valuable not only for their contents but also because of the new words and word-forms in the Elamite language which they contain. Of special value is the list on pp. 42 ff. of Old Persian loan-words in Late Elamite, the number of whch has been greatly added to by the new discoveries. We have thus become acquainted with a large number of proper names, words and word-forms which until now were not attested in Old Persian, and above all we now have a series of Old Persian numerals in

Elamite writing. These are also important for the exact study of Old Persian and Elamite phonology.

The author, already well-known through his excellent *History of Early Iran* (Chicago, 1936), has earned our gratitude by his mastery of the many problems of script and language and kindred matters, and has thereby laid a firm foundation for future research. Not only has he provided a transliteration and translation of all these texts—which are difficult to read and even more difficult to understand—and abundantly annotated them, but has also dealt with particular problems coherently in the introductory chapters. He has also supplied useful tables of the New Elamite writing (which is difficult to read), concerned with its transliteration; and best of all an exhaustive glossary of all the Elamite words both those here published and those in the still unpublished wall-tablets. Dr Cameron is recognised as a cuneiform philologist of wide knowledge and sound judgment. He dissociates himself from the fantastic ideas of Hüsing and his school, and in his thorough and patient method of research approximate rather to the school of Weissbach. We congratulate him for his excellent work and hope for a speedy elaboration and publication of those all-important wall-tablets. JOHANNES FRIEDRICH (Leipzig University).

ABU GEILI. *By* O. G. S. CRAWFORD *and* F. ADDISON. SAQADI AND DAR EL MEK. *By* F. ADDISON. *Oxford University Press for Wellcome Trustees.* 182 pp. 87 *plates.* £2 10*s.*

This volume describes three sites excavated in the years immediately before the first World War as part of the scheme of archaeological research in the area of the Blue Nile and the Gezira in the Anglo-Egyptian Sudan, planned by Sir Henry Wellcome. The volume on the excavations at Jebel Moya has already been reviewed in this journal; this was the chief site and those described in this new volume were excavated as subsidiary ventures of the main expedition and in each case were left uncompleted. The peculiar and lavish way in which Sir Henry Wellcome's work was carried on has already been referred to in Crawford's review of *Jebel Moya* and need not be described again. The difficulties in the way of publication of that site also existed for the sites in this new volume, with the one great advantage that our editor was responsible for the excavation of Abu Geili and has himself been able to describe it. He is in the unique position of being the only archaeologist of the many who worked on Sir Henry's grandiose projects who has been able to participate in their publication.

The site at Abu Geili, which stands on the east bank of the Blue Nile about three miles below the Sennar dam, was found to consist of two entirely separate elements, a Meroitic village and a cemetery of the Fung, the Muslim invaders who came down the Blue Nile early in the 16th century A.D. The dating of this cemetery, and of the pottery with it, is the most important contribution which the book makes to the knowledge of Sudan archaeology. With this secure identification of a very distinctive pottery type, a wide range of sites in the central Sudan can now be dated, and we can begin to reconstruct the material culture of the Fung. The discovery in this cemetery of knives worn at the elbow is of particular interest—this is common practice in the Sudan nowadays and it was presumably introduced by the Fung; earlier graves do not show this usage. It is unfortunate that the dwelling site associated with this cemetery was not found as its excavation would give invaluable information. The suggestion (p. 20) that Bazbosh was too far off to be connected with the graves can be confirmed as a result of the identification of its site by the author and the reviewer in February 1950.

The village, though in the main producing material of known Meroitic type, also produced a certain number of objects of Napatan date. Some of these came from a

level below the lowest floor of the village, from which level also came sherds of Jebel Moya ware. This may be a confirmation of the Napatan date attributed by Addison to much of the occupation at Jebel Moya. Most of the material from the houses themselves is of a type usually considered to be late in Meroitic times and to cover the first two centuries A.D.—Crawford suggests that the earliest finds may be dated a little before the Christian era and the latest after *c.* A.D. 200 but before *c.* A.D. 600, and in the present state of our knowledge this cautious attitude is to be commended. The very late date suggested for the close of Meroitic occupation is unusual, but is based on the occurrence of a grave (the only certainly Meroitic one on the site) at a high level and lying on top of a house wall. This grave contained pottery in no way different from that found in the houses and it would appear, as Crawford suggests, that the material culture of this region remained much the same for a considerable time after the accepted date for the end of the Meroitic kingdom (A.D. 350 or a little earlier). In Meroe itself there is a change in the pottery in the latest graves which may represent an intrusion by the 'Black Noba', and the recent excavations at Soba show that the earliest pottery there had affinities with this material and that the usual Meroitic wares do not occur. But this is no argument that the older types did not persist in areas further south and east and away from the main centres. It should be noted that the pottery from Abu Geili though having much in common with the black ware from the northern sites, such as Faras, has a character of its own in the decoration. The peculiar motif, resembling nothing so much as the rear view of an elephant, referred to on p. 48, is found on a pot from Khartoum (Arkell: *Early Khartoum*, Pl. 106, 7) but is not known further north. The method of decorating by filling the incisions with a red pigment, very common at Abu Geili, and known from Kosti, is rare in the north. It seems therefore that there may be an area of Meroitic culture from Khartoum south with characteristics of its own and with the African element of the strange hybrid that was Meroe very much stronger—it is noteworthy that only very few sherds of the fine painted ware so common at Faras were found at Abu Geili.

Although adding nothing startling to knowledge, the Abu Geili village site has contributed to the clearer picture that is now emerging of the central Sudan in the first few centuries of the Christian era, and both the authors are to be congratulated on the successful publication they have achieved after so many years and with so many difficulties to overcome. The photographs, mostly excellent, but occasionally marred by the appearance of the photographer's shadow, show the excavation to have been in many ways a model one; it is a matter for regret that the high technical standard achieved has not been emulated more frequently in the Nile Valley.

The other two sites described in the book were smaller and of less importance. They were both excavated by Duncan Mackenzie and have been published most ably by Addison from the field notes left by the excavator. The single red brick building which constitutes the site at Saqadi has been referred to frequently in works on the archaeology of the area and unfortunately, since publication was long delayed, the original view of its character (perhaps that of Sir Henry himself?) as a Christian church has been widely accepted. The study which Addison has now made has led him to reject this view and to the opinion, in which the reviewer concurs, that the building was erected by people of Meroitic culture. Unfortunately no date more precise than 'late Meroitic' can be given.

Dar el Mek, the second of the two sites, was a terraced village in the Jebel Moya massif, and consisted of the foundations of both circular and rectangular buildings. It was a poor village and must represent the refuge of some people anxious to avoid the ravages of an invader. The discovery of Fung pottery of the Abu Geili type in the

upper levels shows that the latest occupation did not end before the early 16th century and Addison hazards the guess that the first inhabitants settled there in the 14th century as refugees from the Arabs who swept into the country after the fall of Dongola.

Although there is still much that is obscure in the history of these three sites the material made available in this publication and the views put forward by the authors mark a new point of departure for all subsequent study of the archaeology and history of the Sudan. The standard of production of the book is all that one expects from the Wellcome Trustees and the Oxford University Press, and what makes it unusual amongst excavation reports is that it is readable. P. L. SHINNIE.

HANDBUCH DER ARCHÄOLOGIE : *founded by* W. OTTO, *edited now by* R. HERBIG, *Vierte Lieferung. Munich, C. H. Beck*, 1950. *8vo, paper cover*, XXIV+402 *pp.*. 56 *plates*, 95 *text-figures including folding maps. Price* 45 *DM.*

The German ' Handbook of the Knowledge of Antiquity ' (*Handbuch der Altertumswissenschaft*) is a series of volumes in which an expert on each division of the vast subject, in a ration of many thousand words, undertakes a digest of everything now known about it. A fixed share of the volumes goes to Archaeology, and of the ' Handbook of Archaeology ' series so formed this book is the 4th instalment ; its subject is Europe, with some adjacent regions, from the opening Neolithic to ' about 1000 B.C.' Such a volume is something like an encyclopaedia, but with its subject-matter gathered into continuous long disquisitions, each by a single author. Their aim is to be totally objective, made entirely of ascertained fact and securely-held opinion. But this requires of the authors such sinking of the personal into the oracular, and such condensation of material, that the work can scarcely be redeemed from great dulness, except by the reader's zeal, his thirst for accurate information, and his trust that he is getting it. He can sit to read comfortably nowhere but at the authors' feet.

There are here three authors, and the subject-matter is divided between them thus. Professor Oswald Menghin, formerly of Vienna and now of Buenos Aires, covers a Europe that is extended to the Caucasus and North Africa, but is shorn of Greece and the Aegean islands, which (with West Anatolia and, in the last phase, Cyprus) are covered by Dr Friedrich Matz (Marburg) ; shorn also of Italy and the Central Mediterranean islands, which are the portion of Dr Guido von Kaschnitz-Weinberg (Frankfurt). Matz ends about 1150, with the end of Mycenaean culture ; Menghin arbitrarily at 1000 B.C. exactly, cutting off the rise of his Late Bronze Age in mid-air ; von Kaschnitz-Weinberg, on the other hand, sails on in Italy right through the Early Iron Age, to end only around 500 in more or less historic times. The scheme is thus very clumsy. Post-Mycenaean and Classical Greece of course comes in another volume, and so doubtless will the Late Bronze and Early Iron Ages of the rest of Europe. But what an opportunity for sensible integration has been missed, for the sake of division according to customary academic fields ! These conventional paddocks, with the high and almost windowless walls which in eighty years the universities of nearly all Europe have built round them, are the curse of archaeology. The scholastic mentality that maintains them, unfortunately, seems unaware that it is dead.

Of the three authors the most successful is without doubt Matz. Long a devoted student of the prehistoric Aegean, and a follower of Karo whose first draft for this section he took over in 1939, he really does inspire one's zeal and trust. His documentation runs copiously from the beginning to 1939 and after, just touching Schaeffer's *Stratigraphie Comparée* of 1948, and Hutchinson's ' Notes on Minoan Chronology ' in that

year's ANTIQUITY. His accuracy is high, his judgment cautious but keen: see, for example, pp. 231–2 on Middle Minoan dating, p. 280 on the age of the 'Treasury of Atreus' at Mycenae, and the Addenda on Troy, Thessaly, etc., at pp. 306–8. To a reader prepared for scholars' German, who wants a close-packed but orderly account of the prehistoric Aegean world as discerned by archaeology up to the decade 1939–48, Matz has laboured worthily to give the best that in 130 pages can be given.

On Italy and the adjacent islands, von Kaschnitz-Weinberg also has worked from an originally pre-war text: his 'Aeneolithic' chapter and its Neolithic opening do not quite take in Bernabò Brea's recent writings, nor the Gaudo cemetery at Paestum. On the whole they are rather competent than masterly. He works in like manner through the North Italian Bronze Age (pp. 342 ff.; Terremare as re-assessed by Säflund, Patroni, and MacIver in ANTIQUITY for 1939) and its Central European elements (much on pottery, but not really decisive, though Urnfields seem to start earlier south of the Alps than north of them; much also on bronzes, but sometimes too decisive: Peschiera fibulae (p. 349) are uncritically brought direct from Merhart's home for them in Hungary, while their appearance in Mycenaean Greece is curtly dismissed as a 'détour'). The contemporary Apennine culture, with its Balkan elements (pp. 354 ff.), is more steadily done, but one misses the fervour of a Rellini, the fire of a Laviosa-Zambotti: von Kaschnitz-Weinberg among the Apennines is the serious German tourist. But the tour stops short of Sicily: his bare two paragraphs on the Sicilian Bronze Age (pp. 362–3), with its Mycenaean imports and inspirations, are unworthy of the volume. His Nuraghic Sardinia, too, is ruined by division between two chapters (pp. 332 ff., 364). His last chapter, on the Italian Early Iron Age (pp. 364 ff.), is devoted to painstaking examination of the field, but by the light of dogmas too seldom critically justified. We are taken successively through 'Cremating and Inhuming Italici', the latter displayed from the Balkan-Danubian-Merhart angle, the former brought from Hungary or even Rumania (the much-disputed Coste del Marano hoard is dated '12th century—1000 B.C.' with no hint that it is disputed at all); the Villanovan culture (beginning now '925 B.C.', he says, rightly rejecting earlier figures but considering no later ones except Åkerström's which are 200 years later, and so missing the whole case for a middle view); the Early Etruscan (Tomba del Guerriero '725', but Greek background for chronology barely sketched, and origin of Etruscans not discussed); and the North and East Italian cultures (treatment broadly similar). Sicily and the islands are again dismissed in two pages, and the disquisition ends with a plate of the Capestrano Warrior. The interest of the article, in fact, is that of a museum-piece showing what German archaeological thought about ancient Italy *was*, in the second quarter of the 20th century A.D.

Menghin, on the rest of Europe, is no less oracular. But the *Kulturkreis* doctrine of Vienna anthropology, applied with such skill to prehistory in his *Weltgeschichte der Steinzeit* 20 years ago, is here applied no less ruthlessly, and with far less space for charm; and the effect is unashamedly artificial. Neolithic Europe is divided between an Eastern *Kulturkreis*, a Western (including the British Isles, Italy and Sicily, and related to the North-African), a Nordic, a Danubian, and a Pontic-Caucasian (with also the Aegean-Anatolian covered by Matz): these *Kulturkreise*, and their successors of the Bronze Age, each with its own material, aesthetic, spiritual, racial, and linguistic content, and frontiers sharply drawn, are the pre-fabricated units of the story. Each is described from its material defined as 'art', with the implications in terms of culture-history. The documentation is widely spread, and items for 1939–48 have been added by Dr V. Milojčić, mainly for Central and S.E. Europe. But its real value shrinks as it gets beyond the familiar German sphere; and even within it, the selection is marred by bias. Thus

for the statement that the Nordic *Kreis* can from the beginning of the Bronze Age be safely called ' Germanic ' (p. 9) the authority of Kossinna (1928) is given without any sign that this is controversial—except the fact of disagreement by one American, C. S. Coon (unexplained); and for the inclusion in it of N.W. Germany, still more controversial, a paper by Jankuhn (1939) is alone quoted, without any mention of the opposite conclusion reached at great length in 1941 by Sprockhoff (*XXXI B.R.G.K.*, II, 1–138). And when on p. 81 we are told that ' in its existence-struggle against the advancing North-current, the West-European culture-circle of the Bronze Age was apparently no longer capable of creating a great independent art ', there is no documentation at all; perhaps just as well. A minor absurdity illustrates this bias against things non-German even better: on p. 84, the Bohemian Aunjetitz culture is defined without mentioning Bohemia! Yet Czech literature is honourably cited in the footnotes; such matters as the Near Eastern and Mediterranean influences in Central and Northern Europe, too, are sensibly presented.

The trouble is, that the even surface imposed by the ' Handbook ' manner smooths good and bad indifferently together. It also masks, superficially, the gaps in the author's knowledge of his subject—due partly to bad contacts with foreign literature, but sometimes to bad grasp even of literature specifically referred to. Thus the footnote to his warning on p. 6, that Egyptian faience beads are in Egypt *not* datable reliably, carries without further comment the reference ' Archaeologia 85 (1935) 203–252 ' as though it justified his scepticism; whereas really, of course, this is Beck and Stone's famous article relating British finds of these to examples which *are* datable reliably—a fact which he nowhere mentions. And on the dating of Western megalithic monuments (pp. 99–100) he cites various views (down to the La Tène theories of Stonehenge and Tressé) without attempting any critical assessment of them, remarking simply that ' the dating of the West-European, above all the British megaliths is on the whole a difficult problem '. On the whole an easy escape! This is what comes of expecting one man to be an ' expert ' on more than he can encompass.

The second half of Menghin's disquisition is on Neolithic and Bronze Age architecture. Much of it stems from the well-known monograph by Franz Oelmann. (Those oblong stone structures in Brittany, by the way—Mané-Pochat-en-Uieu, Mané-Tyec, and Mané-Clud-er-Yer—which Piggott, in ANTIQUITY 1937, 445–6, took to be long barrows, were taken by Oelmann, in *Germania* VI (1922), 105, to be long *houses*: ' assembly-lodges ' or ' chiefs' halls '. It is worth noting this in connection with the recent article by Childe, ANTIQUITY 1949, 129–35, on the same issue in the Neolithic North). These chapters make a clever essay in classification: to my mind over-ingenious and over-systematic, like all Menghin, but certainly the best act in his performance here, and with many illustrations as well as references, ranging over the chief constructional types of these periods in Continental Europe. How much better it would have been if published as a monograph made all the author's own, and not as part of a Handbook which expects one to believe everything in it! For, taken all round, this volume will best serve those who will *not* believe everything in it—until they have looked up what work it rests on and what has followed it, and formed their own opinions. Take it as a monument, and all its stones whether good or bad are dead, because immovable; take it as a quarry, and they can be dragged out and put to living use. The easier the authors and editor have made this needful task, the better they have worked. But the *Handbuch* will only have been worth while if enough readers see that the task is needful. Will many? Let us hope so.

CHRISTOPHER HAWKES.

EXPLORATION AND EXCAVATIONS : 1. Prehistoric and Protohistoric Period. *By* B. B. Lal. *Archaeology in India* (1950), 17–50 ; xii *pl.*

THE PAINTED GREY WARE OF THE UPPER GANGETIC BASIN. *By* B. B. Lal. *Journ. Royal Asiatic Soc. Bengal* (Letters), xvi (1950), 89–102, viii *pl.*

SISUPALGARH 1948 : An Early Historical Fort in Eastern India. *By* B. B. Lal. *Ancient India, no.* 5 (1950), 62–105, 12 *figs.*, xxiv *pl.*

These three papers, by a member of the staff of the Archaeological Department of India, are a most welcome indication that the new Dominion is tackling its archaeological problems with competence and imagination. Mr Lal's first paper is a convenient summary of the present state of our knowledge of the most ancient India, but in its treatment of the Indian ' Dark Ages ', and of the rock-paintings in Central India, it includes original material not easily accessible elsewhere, and a valuable discussion of the problems involved.

In this second paper, Mr Lal presents the results of his field-work in the Upper Ganges, undertaken with a view to filling the chronological and cultural gap between the last days of the Harappa Culture and the dawn of Indian history in the 4th and 3rd centuries B.C. As a starting-point, he took a very distinctive type of painted black-on-grey pottery which had been found in the earliest levels of the town-site of Ahichchhatra (Bareilly) during the 1940–44 excavations, with a probable date of somewhere before 300 B.C. Mr T. G. E. Powell and I saw this pottery on the site early in 1944 and were struck by its potential importance, and shortly afterwards we looked for and found typical sherds of the same ware in soil deriving from the lowest levels of the ancient town-site of Muttra. We also searched, unsuccessfully, such surface exposures as existed in the ' tells ' representing the ancient sites of Indrapat and Tilpat. These are two of the five villages which were a famous source of contention between the Pandavas and the Kauravas in the *Mahabharata* : Mr Lal has triumphantly succeeded in finding, in trial excavations, the painted grey ware not only at these sites, but at two more of the five villages, Baghpat and Panipat, and at Hastinapura itself, the capital of the Kauravas. Another ten sites in Uttar Pradesh (former United Provinces), Indian Punjab, Patiala and Rajasthan have also produced this pottery. At two sites, Chhat and Panipat, the painted grey ware appeared to belong to the earliest occupation of the sites, while at the former, and at Tilpat and Hastinapura, it was stratified well below the pottery classified by Wheeler as ' Northern Black Polished ' ware, which at Taxila (Bhir Mound) could be related to coins of Alexander, and its range of date fixed with a central point of *c.* 400 B.C. Mr Lal therefore concludes that we may reasonably assign the painted grey ware to somewhere in the first half of the first millennium B.C. This is ' closing the gap ' between the date of the probable first arrival of Aryans in the Punjab (second half of second millennium B.C.) and the beginnings of Indian history in a most satisfactory manner, and its bearing on the date of the composition of the *Mahabharata* may clearly be most important.

In East India, Mr Lal has attacked the same problem from another angle, following up Dr Wheeler's work at Arikamedu and Brahmagiri. These excavations established the existence of a characteristic rouletted ware, which was in fact a native imitation of first-century Arretine ware imported from the Mediterranean ; such pottery had also been found at the well-known stupa site of Amaravati in the Guntur District of Madras. Mr Lal chose for excavation a site at Sisupalgarh near Bhuvanesvar in Orissa Province, 220 miles southwest of Calcutta and over 700 miles north of Arikamedu. The site was found to have four main periods of occupation, with the lowest occurrence of rouletted ware (of *c.* A.D. 50) near the middle of the second phase—the site appears to have been

occupied from the beginning of the 3rd century B.C., with black-and-red ware of the type characteristic of the Chandravalli Megalithic Culture in the earlier layers, and to have continued into the mid 4th century A.D. The defences were studied in detail, and their constructional history worked out.

The interim report on Sisupalgarh is a model of its kind, admirably arranged and illustrated. The draughtsmen of the Department have taken up the ' Wheeler tradition ' of fine archaeological illustration and are developing it along the individual lines needed for the peculiar problems of Oriental sites : the drawings are often anonymous, but it is pleasant to be able to congratulate at least Mr Raghbir Singh and Mr B. Saran for their work—the former's notable drawing of the section through the Harappa defences, published in an earlier number of *Ancient India*, deserves to rank among the really outstanding performances of its kind.

Several of us, the reviewer in particular, have been sharply critical of the standards of excavations, and excavation or field-work reports, of oriental sites. This criticism has been prompted by the belief that the exacting standards built up in Europe on the basis of such traditions as that of Pitt-Rivers of the German Limes-Commission could perfectly well be applied east of Suez if only the problem was approached in the right way. And it is now clear that, with such work as that of Mr Lal reviewed above, India is applying these standards and is leading the way among the Eastern nations in the scientific examination of her past. In her task she will have the support of all who are concerned with the establishment and maintenance of high standards of scholarship and technique in archaeology.

STUART PIGGOTT.

SITES AND PRESERVATION OF PREHISTORIC BASKETRY AND WOVEN FABRICS. *By* E. VOGT. *Ciba Review, no.* 54. *Basel, January,* 1947.

This is a contribution, by Professor E. Vogt, Keeper of Antiquities at the Swiss National Museum in Zürich, to what appears to be an exceptionally intelligent form of trade journal. This number, which deals with ' The Basketry and Woven Fabrics of the European Stone and Bronze Ages ', should be in the hands of all archaeologists and students of textiles. In the first sections the writer discusses the differing conditions under which ancient textiles are found and gives instructions for their examination and after care. Preservation in lakes and peat-bogs, salt mines, sand and dry ground, as well as by contact with metals are all well discussed ; the note on the difference between the action on textiles of bronze oxide with its gentle permeation and that of the stronger ferric oxide, which indeed sometimes entirely replaces the fibres is of peculiar interest.

The advice that follows, on the examination of ancient textiles is excellent. We cannot agree, however, that ' it may be taken for granted that the delicate fabrics of vegetable origin are of flax fibres '. The Badarian textiles, at first thought to be of flax were afterwards discovered to be of some other plant fibre, and more recently, the latest investigator of the Voldtofte Bronze Age linen, Magister Køie, has declared that the fibre samples examined by him are ' all pure nettle fibre[1] '. Over wool the author himself issues cautions, citing the amusing controversy over the hairs in the yarns of the north, once thought to be due to an admixture of roe-deer hair with wool, and now believed to be wholly due to the coat of a primitive sheep ' the fur of which still contained the long stiff bristles ', a very kempy fleece, as we should say. Still, there has been notable progress in the study of fibres for other purposes than archaeological study and we may hope to gain more light from it in the future.

[1] M. Hald, ' The nettle as a culture plant '. *Folk-Liv* 1942.

One problem that arises acutely in the study of the Lake Village textiles, their black and ' charred ' condition, has not yet, we are told, been scientifically examined. ' It is hardly conceivable ', he says, ' that they should have been actually burnt '. I remember the same problem arising over the Meroitic cottons discovered by Dr Reisner ; there was no evidence for a fire, and yet the textiles were all black. The possibility of ' damp carbonisation ' was raised, but there seemed then as here, no scientific solution available.

After these sections the author gives an excellent résumé of his own remarkable work on the basketry and textiles of the Swiss Lake Villages, with short accounts of the finds of the Iberian cave culture, the Danish Bronze Age, the Swedish cloak from Gerumsberget, the fabrics from the Dürrnberg salt mines and other of the period under review.

There is one minor point on which I venture to differ from Professor Vogt. In a closing note on *Embroidery and Plaitwork Ornaments* he questions whether the sewn ornament on neck and sleeves of the Bronze Age woman's jacket from Skrydstrup, Denmark[2], is embroidered. The sleeve decoration in question is in a stitch known to modern embroiderers as ' Indian-ground ' stitch ; and the lace-like buttonhole filling of the neck is further ornamented with a corded oversewing ; though no doubt this buttonholing does derive, as he points out, from the primitive ' knotless net ', it seems to me that when used thus it must be regarded as true embroidery. GRACE M. CROWFOOT

THE DESTRUCTION AND RESURRECTION OF POMPEII AND HERCULANEUM. *By* EGON CAESAR, CONTE CORTI. *Translated by* K. *and* R. GREGOR SMITH. *London, Routledge and Kegan Paul,* 1951. *pp.* x+220. 38 *photographs,* 3 *line drawings.* 25*s.*

This book—a translation of the German edition of 1943—professes to be a popular and comprehensive summary of the course of the eruption of Vesuvius in A.D. 79 and of the re-discovery and resurrection of the classical sites buried by that eruption. Had the author stuck strictly to his professed aim, the result would have been more homogeneous and less misleading. The last four out of the seven chapters and that part of the third chapter which deals with the events of A.D. 79, though discursive and anecdotal in style, are graphically written and show signs of original investigation, notably in the Vienna State Archives. There is no such extensive history of the excavation of the two towns to be found elsewhere in English.

The author has, however, prefaced these chapters with a history of the towns from the period of Oscan settlement down to Roman times, and, unfortunately, in the realm of ancient history and archaeology, he is less at home and less happy. The history of Pompeii and Herculaneum depends almost entirely on archaeological evidence, references to them in ancient literature being few. It is, then, essential in treating of them to refer to buildings clearly, to describe them accurately, and to distinguish between fact and inference, especially in disputed matters. Count Corti does not always do these things, and since he gives only three plans—and those poor ones—the reader without prior acquaintance with Pompeii and Herculaneum, would find the early chapters confusing and, in places, downright misleading. The plates are numerous and of good quality and are the best feature of the book, presumably explaining its high price. Even so, they do not give a balanced picture of the remains of the towns, but concentrate too much on well-known works of art from the older diggings, and show too little of the newer excavations with their wealth of information on everyday life.

R. C. CARRINGTON.

[2] H. C. Broholm and Margrethe Hald, *Costumes of the Bronze Age in Denmark*, p. 91, figs. 121–130.